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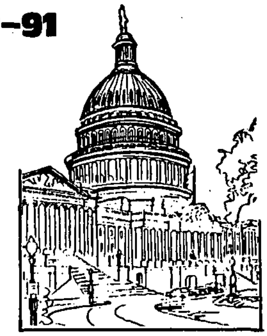

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ARTICLES

AMERICAN FEDERALISM, WELFARE POLICY, AND RESIDENTIAL CHOICES

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The relationship between welfare benefit levels and the residential choices of the poor raises two issues for federalism in the United States. Do state benefit levels affect the residential choices of the poor? Do residential choices of the poor affect the level at which a state sets its benefit levels? Empirical studies have seldom studied the interconnection between these two issues. This research estimates simultaneously the mutual effects of welfare benefits and poverty rates while controlling for other economic and political variables. When benefit levels become high, the size of the poverty population increases. Conversely, when poverty rates become high, benefit levels are cut. The findings are consistent with the claim that state-determined benefit levels distort policy and residential choices.

Who should determine welfare policy in a federal system? The debate on this question has focused on which level of government can best meet citizen preferences. Some have argued that allowing states to set welfare levels increases the number of combinations of taxing and spending available, so that citizens have more choice in selecting places matching their preferences (Pauly 1973; Tiebout 1956). Others have maintained that if states bear the responsibility for redistributive programs, the free rider problem will be severe; states will consequently limit their welfare provision, hoping that some other state government will take care of it (Brown and Oates 1986; Ladd and Doolittle 1982; Peterson 1980).

This debate actually hinges on two empirical issues. The first concerns the effect of welfare policy on residential choices. Some have found that welfare policies have little influence on individuals' decisions about where to live and that people migrate (or remain at home) mainly for reasons of family or job opportunities (Fields 1979; Long 1974). Other studies find evidence that states offering higher welfare benefit levels act as a magnet, attracting poor people who would not otherwise move there and retaining poor people who might otherwise move away (Blank 1985; Gramlich and Laren 1984).

The second issue concerns how states determine their welfare policies. The dominant approach to state policy making holds (often implicitly) that politicians respond to political forces and economic conditions within their state (Jennings 1979; Plotnick and Winters 1985). A contrasting approach emphasizes that states are engaged in economic competition and that policy makers establish policies to enhance their state's relative economic

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position (Oates 1972; Peterson 1980).

Resolution of these issues has implications for selecting the appropriate level of government to make welfare policy. If poor people do not make residential decisions in response to state welfare policies and states ignore other states' benefit levels when determining their own levels, the states may be the appropriate policymaking unit. However, if states' welfare policy decisions are significantly influenced by the economic competition of their sister-states and residential choice of those at risk of welfare dependency is influenced by state policy, the federal government may be the more suitable party.

Although these debates are long-standing, empirical studies have seldom studied their interconnection (but see Cebula 1979; Gramlich and Laren 1984; Southwick 1981). On the one side, studies of welfare policy (usually conducted by political scientists) have rarely considered the possibility that the size of the low-income population might itself be affected by welfare benefit levels (Jennings 1979; Plotnick and Winters 1985). On the other side, studies of the residential choices of groups at risk of welfare dependence (generally conducted by economists) have usually ignored the possibility that migration could affect welfare benefit levels (for a review see Cebula 1979; Schlottmann and Herzog 1981). Our research estimates the mutual effects of welfare benefits and migration levels on each other. The findings lend empirical support to the view that state-determined benefit levels distort both policy and residential choices.

Previous Research

State Policy Making

The dominant approach to state policy-making research has been based on the assumption that politicians are responding to political forces within the state (Fry and Winters 1970; Plotnick and Winters 1985). In formalizations of this approach it is assumed that state officials are designing policies in order to attract the support of the median voter. Within this framework, policy outcomes can be shaped by political factors such as party competition or degree of electoral mobilization, because these factors help determine which groups are best able to muster voter support for their position.

Policies can also be shaped by such social and economic variables as per capita income, degree of urbanization, and population density. In early research on state policy these variables were misconceived as purely economic factors that determined public policy independently of a state's political system, and a good deal of debate was devoted to the relative importance of economic versus political determinants of public policy. But as Plotnick and Winters have made clear, "the conflict between economic and political explanations of redistribution is an artificial one" (1985, 471). Within the framework of the median voter model, economic variables can best be understood as indicators of voter preferences. For example, rural voters are expected to demand more roads per capita, and increased population density can be expected to result in demands for more police and fire protection. As long as one realizes that economic variables help identify political predispositions, it is not difficult to incorporate them in a median voter model of policy formation.

Much of the popularity of this model is due in fact to the flexibility with which it can be used. Indeed, different studies using the median voter model have made exactly opposite inferences. For example, in some uses of the median voter model it is assumed that the higher the proportion of poor in a state, the greater the demand for welfare services (Peltzman 1980). Others have argued that taxpayers will increasingly oppose welfare provision because tax burdens increase as the percentage poor increases (Gramlich and Laren 1984; Plotnick 1987; Plotnick and Winters 1985). If increasing poverty rates can be said—other things being equal—to cause vote-conscious politicians to raise benefit levels as well as to lower them, then the median voter model is simply tautological.

A second approach emphasizes that states are engaged in economic competition and assumes that state policy makers attempt to enhance the economic performance of their state (Peterson 1980). In pursuit of high economic performance, they design public policies so as to be attractive to firms, productive workers, and investors. This approach can be formalized by constructing a model assuming that state policy makers seek to maximize state property values (much as executives of a firm are assumed to be interested in maximizing profits). Unless policy makers hold to this objective, their states risk economic decline, loss of tax revenues, and—in the extreme case—inability to raise capital funds in the bond market (just as a firm that does not maximize profits risks devaluation of its stock, takeover attempts, and bankruptcy).

This view of state policy making gives more precise predictions about welfare policy choice. Policy makers are more likely to assist the poor if their taxable wealth is already high relative to those in other states. Under these circumstances a higher level of welfare provision can be offered at tax rates at or below the level pertaining in other jurisdictions. The needy can be more easily accommodated without giving productive firms and productive citizens incentives to move elsewhere. But if benefits exceed other states' benefits by too much, policy makers will act to reduce them in order not to attract poor people. And if poverty rates in a state are high and increasing, value-maximizing policy makers will be even more inclined to reduce benefit levels.

The value-maximizing and median voter models can be reconciled by assuming that economic conditions in a state have significant political consequences (Fiorina 1981; Hibbs 1987; Kernell 1986; Kiewiet 1983). If voters judge political incumbents according to how well the state economy is performing, one way political leaders can gain votes is to promote economic growth or, more generally, maximize property values. Economic prosperity need not be all politicians consider when formulating politically popular welfare policies. In addition, their calculations may be affected by such factors as the degree of party competition in the state, the extent to which low-income citizens are politically mobilized, and the liberality of the state's political culture.

Formulated in these terms, the question is no longer a matter of which model is correct. Instead, the two models are combined to generate more specific hypotheses than are ordinarily generated by the median voter model. In particular, the property-maximizing model is used to specify the vote-maximizing model by identifying a set of economic factors with significant political consequences. Instead of treating voters as simply liberal or conservative, rich or poor, altruistic or self-interested—as many applications of the median voter model do—this conceptualization regards the median voter as economically sensitive, as most studies of retrospective voting have shown him or her to be. If a state is doing well economically, incumbents tend to be rewarded. If a state is suffering economic decline, voters are more likely to call for a change (Chubb 1988).

Interstate Migration

Most studies of the effects of welfare benefits on interstate migration have ignored these issues. Instead, they have typically taken welfare benefits as a given and then examined their effects on the residential choices of welfare-sensitive populations. Their findings have depended mainly on the time period during which the data they analyzed was collected. The first and largest wave of studies was undertaken in the late sixties and early seventies in the context of the debate over the family assistance plan proposed by President Richard Nixon. The administration justified its proposal for more standardized benefits partly on the grounds that large differences in welfare benefits were encouraging the movement of potential recipients from low-benefit to high-benefit states. Critics responded by pointing to studies showing that the amount of interstate migration attributable to differential welfare benefits was either statistically or substantively insignificant.[1]

The data for these studies was collected in the fifties and sixties, a time when many states did not provide welfare benefits until newcomers had met a 12-month residency requirement (Brown and Oates 1986). But in 1969 the Supreme Court (in *Shapiro* v. *Thompson*) ruled that states could not constitutionally deny benefits to new residents of the state. After that date welfare benefits had to be made available as quickly as applications could be processed. Consequently, differences in state welfare benefits may have had a more substantial effect on residential choice after 1970 than they did previously.

Two important recent studies using data collected since 1970 (Blank 1985; Gramlich and Laren 1984) suggest that the conclusions reached at the time of Nixon's proposal may no longer apply, but each has limitations that leaves the issue unresolved.[2]

Research Design

In presenting our analytic procedures, we will discuss how our methodology improves on prior studies and describe the nonrecursive model and the data we use for estimation. We conclude with a discussion of our findings and their implications for welfare reform.

Benefit Levels

Welfare policies differ dramatically among the states. Although some welfare policies are determined at the federal level, the states exercise considerable discretion over how the funds are spent and the level of benefits a family may receive. For example, California provided welfare recipients in 1985 with as much as five times as large a welfare stipend as did Mississippi, the least generous state. Such policy differences among the states were not just the peculiarities of one or two states, nor have they been gradually disappearing as state governments gain in professional responsibility and fiscal capacity. Instead, as much variability in benefit policies existed in 1985 as in 1940 (see Table 1). Roughly speaking, in 1985 a state one standard deviation above the average was providing nearly $300 per month more for a family of four than a state one standard deviation below.

Significantly, this variability in benefit levels has continued even though the value of welfare benefits in real dollars has been declining since 1970. Benefit levels in the typical state are today only two-thirds of their 1970 level and are in fact below the level they reached in 1950 (see Table 1).[3]

The food stamp program, which is funded entirely by the federal government, has mitigated both the decline in welfare benefits and the degree of interstate variation. Food stamp and cash payments in 1985 are roughly the same as cash payments were in 1960, a time when the food stamp program had yet to be inaugurated.[4] In fact it has been argued that food stamps, originally thought to be an addition to cash benefits, have become a substitute instead (Moffitt 1988).

Table 1. Variations in Welfare Policy among the States: Benefit Levels in Adjusted Dollars, 1985

Welfare Payments	1940		1950		1960		1970		1975		1980		1985	
	Mean	Variation[a]	Mean	Variation	Mean	Variation	Mean	Variation	Mean	Variation	Mean	Variation	Mean	Variation
Average monthly AFDC payments	231	.33	314	.34	413	.32	450	.34	381	.35	319	.34	292	.35
Maximum monthly AFDC payments														
3-person family	—	—	—	—	—	—	—	—	462	.33	388	.34	326	.34
4-person	—	—	—	—	—	—	597	.36	542	.34	449	.36	389	.33
Maximum combined AFDC-food stamp payments														
3-person family	—	—	—	—	—	—	—	—	567	.19	487	.19	439	.18
4-person family	—	—	—	—	—	—	712	.21	687	.19	587	.19	540	.17

Note: For sources, see Peterson and Rom 1988c, Table 3.

[a]Coefficient of variation.

The establishment of the food stamp program has also reduced the interstate variation in total benefits by roughly one-half, as compared with the variation in the pre-food stamp period (see Table 1). This occurs because the entirely federally funded food stamp program gives higher food stamp benefits to recipients in states where cash benefits are lower. However, combined food stamp and cash benefits in the most generous state still remains more than twice as high as in the least; the highest combined benefits for a family of four are $757 per month, the lowest $369.

As a measure of welfare benefits we chose the combined maximum cash and food stamp guarantees as the broadest available measure of welfare policy and thus the likeliest to be salient to potential recipients (U.S. House of Representatives, Committee on Ways and Means 1986, 393). Also, maximum welfare guarantees are directly controlled by policy makers.

The Population Most Sensitive to Welfare Benefit Levels

One of the most difficult problems to solve in studies of the relationships between welfare policy and residential choice is how to measure the net change due to migration in the size of the population at risk of becoming dependent on welfare. In order to determine the effects of policy on residential choice, one needs to measure group size changes that are due to interstate migration, as distinct from change in the characteristics of individuals living within a state. The most common indicators of this group have been *black* and *currently receive welfare.* Each has distinct disadvantages as an indicator. Eighty-five percent of the black population do not receive Aid to Families with Dependent Children (AFDC) payments, and more than one-half of AFDC recipients are not black (U.S. House of Representatives, Committee on Ways and Means 1986). In addition, the size of the within-state recipient population may fluctuate as a result of policy change. As benefits increase, more individuals become eligible for assistance and more individuals may find it worth their while to incur the information costs, bureaucratic hassle, and public stigmata that accompany an application for benefits (Moffitt 1987). Because welfare policy changes can affect the size of the within-state welfare population in these ways, it is difficult with aggregate data to distinguish any of these within-state effects from changes in interstate migration patterns. Consequently, we did not rely primarily on either variable.

The deficiencies of the measure we have emphasized instead—changes in the poverty rate within a state—are outweighed by its advantages. First, it is a much better proxy for the welfare-sensitive population than percentage black. About 33% of the population officially described as living in poverty receive AFDC payments at any given point in time (Danziger 1988, tbl. 5). Many poor people—not just welfare recipients—are likely to be sensitive to changes in welfare policies. Contrary to the myth that the welfare system affects only a well-defined "underclass" consisting of welfare recipients dependent on income maintenance programs for long periods of time, most of those who receive welfare in any given year leave the rolls within a year (Bane and Ellwood 1983). In other words, the rolls are continuously churning, thereby involving a much larger percentage of the low-income population than is usually realized.

If the poverty rate is a good indicator of the population most susceptible to changes in welfare policy, its weakness is that changes in this variable can occur for reasons other than interstate migration. For one thing, if employment opportunities and earnings levels change within a state, within-state poverty may change without any migration at all. We have addressed this problem by including appropriate controls for changes in state economic conditions.

A second problem is less easily addressed. It may be that changes in welfare policies themselves affect the within-state size of the poverty population. Two possible effects may occur. First, in several states, welfare benefits combined with earned income could lift a family above the poverty line. For example, in California, the most generous state, a family of four in 1985 could have earned $6666—provided that child care and work-related expenses equalled that amount—and still have received the maximum of $8376 per year. The combination of benefits and earned income could technically have lifted the family above the poverty line of $10,614.[5] This rarely happens, however; 87% of families receiving AFDC benefits do not report any other source of income. Only 1.5% of women enrolled in the program have full-time jobs, with another 3.4% working part-time (U.S. House of Representatives, Committee on Ways and Means 1986, 392).

The second possibility is that increases in benefit levels may actually increase the size of the poverty population. Studies differ as to whether income maintenance policies encourage the formation of female-headed households, the demographic group at greatest risk of being poor (Plotnick 1987; for a contrary finding see Ellwood and Bane 1986). Studies also differ as to whether higher benefits may reduce incentives to work and thus discourage recipients from obtaining entry-level jobs and better skills that could lead to escape from poverty (Danziger, Haveman, and Plotnick 1981). To the extent that these relationships exist, poverty rates within a state could change as a function of changes in welfare policy without any migration.

Selection of Time Period

In constructing a data set that took into account the sluggishness with which both human migration and political decisions respond to environmental change, we organized our data into three time periods of five years each: 1970–75, 1975–80, and 1980–85.[6] Although the selection of time periods was determined in good part by the availability of poverty data and other demographic data, these data constraints did not force us to make particularly restrictive assumptions. Changes in the distribution of poverty occur only slowly, while benefit levels are only occasionally adjusted by the states. Five-year periods may not perfectly capture the lags between changes in welfare and poverty, but they should be long enough for us to observe any connection that exists. We use data beginning in 1970 because only after the 1969 Supreme Court ruling (*Shapiro* v. *Thompson*) could poor people move freely and still remain immediately eligible for welfare benefits.

The Unit of Analysis

Our units of analysis consist of the 48 contiguous states plus the District of Columbia for each of the three time periods specified above. The state is obviously the best unit of analysis for estimating the effects of poverty rates on state welfare policies, but it has certain disadvantages for estimating the effects of welfare benefits on interstate migration. We chose not to use individual-level data because it precluded our examining the simultaneous effects of poverty rates and benefit levels on each other and because available individual-level data sources require the use of very small sample sizes and do not permit analysis of change over regular time intervals.

A Simultaneous Model of Change

We first present the model used to estimate changes in benefit levels, then the model used to estimate changes in poverty rates.[7]

Changing Welfare Benefits

The following equation was used to estimate changes in benefit levels:

$$\Delta B_{it} = a_0 + a_1(P_{ij}\Delta P_{it}) + a_2 B_{ij} + a_3 PI_{ij} + a_4 T_{ij} + a_5 C_{ij} + a_6 P_{2it} + a_7 P_{3it} + e_{it},$$

where ΔB = change in welfare benefits; P = poverty as a percentage of the population; ΔP = change in poverty; B = welfare benefit levels; PI = levels of political competition and participation; T = levels of tax effort; C = tax capacity level; P_2 = period 2 (1976–80)—dummy variable; P_3 = period 3 (1981–85)—dummy variable; e_{it} = residual; i = lower 48 states and the District of Columbia; j = year, where j = 1, 2, or 3, and 1 = 1970, 2 = 1975, 3 = 1980; and t = time period, where t = 1, 2, or 3, and 1 = 1971–75, 2 = 1976–80, 3 = 1981–85.

Poverty. We hypothesized that higher poverty levels would gradually induce policy makers to lower benefit levels over time. We also hypothesized that policy makers are sensitive to changes in poverty levels. If poverty levels were rising, pressure to cut benefits would immediately increase. To test these hypotheses, we specified our model in two different ways. In one equation, initial poverty levels and changes in poverty rates were both entered separately. In the second, poverty levels at the end of the period were used as indicators of policy makers' expectations of what poverty levels would be. Preliminary analysis suggested that neither hypothesis could be rejected. Because multicollinearity among the variables precluded inclusion of all three variables within one equation, we chose the expectations model, primarily because it was theoretically more satisfying and it produced a better fit. (The results from the analysis using the initial poverty levels and the change variable are found in Peterson and Rom 1988c, app. B.)

Economic conditions. The model also takes into account the importance for welfare policy of other economic factors (Peterson 1980; Plotnick and Winters 1985). In general we hypothesize that the more prosperous the state, the less constrained policy makers are when it comes to financing social welfare programs. The same tax rate will generate more revenue per capita, making it easier to finance redistributive programs without providing disincentives to firms and taxpaying households. We used indicators of the level of economic prosperity in the state at the beginning of the period rather than indicators of changes in the state economy during the period, because we assumed that policy makers reacted slowly to environmental change and that economic factors would influence policy gradually over a five-year period. We originally included unemployment rates, employment levels, per capita income, and tax capacity in our analysis; in the final equation we retained only tax capacity, a summary indicator of the overall taxable wealth of a state—the one whose coefficients remained stable and significant.

Political institutions. Institutions structuring the state's electoral politics were also expected to influence state welfare policy. Drawing on the work of V. O. Key (1949), Jennings (1979, 415) observed that "close interparty competition provides strong incentives for both parties to formulate policies for the benefit of the 'have-nots' "; if politicians love the poor, it is because there are so many of them who can potentially vote. But party competition alone cannot explain variations in state welfare policies: as Jennings (1979, 416) concludes, "The generosity of a state's social welfare policies depends upon the organization and mobilization of the lower classes in the election process"—which requires more than just party competitiveness.

Because these institutional conditions influencing welfare policy appear to be interrelated, we constructed an index combining measures of party competition and class mobilization. The partisan balance between the Democratic and Republican parties in the state legislature was chosen as the best indicator of the degree of institutionalized party competition influencing state welfare policy. Measuring class mobilization was more problematic, since good-quality data on the effects of socioeconomic background on voter turnout and partisan choice is lacking at the state level. As a substitute, we used voter turnout in gubernatorial elections. States with high voter turnouts probably have higher class mobilization and are more likely to have at least one party in favor of broadening the welfare state.

Political culture. The state's political culture is a third factor expected to influence welfare policy. A number of studies suggest that states exemplifying moralistic political cultures will be more willing to tax themselves to pay for social welfare programs, simply because they believe it is the right thing to do (Fenton 1966; Sharkansky 1969). The best available measure of this aspect of a state's political culture is a measure of how much a state, given its economic resources, is willing to tax itself. We hypothesized that the higher a state's "tax effort," the less we would expect it to cut welfare benefits even when controlling for other political and economic changes. Because culture is expected to shape policy only gradually and imperceptibly, we used a level variable, rather than a change variable, to estimate its impact.

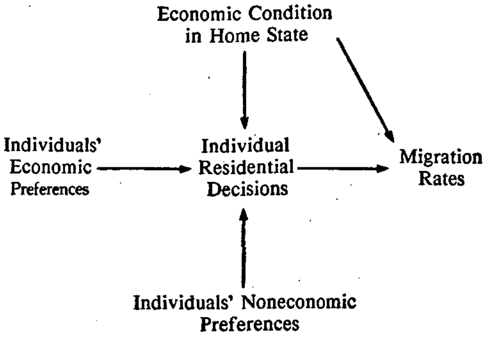
State "exposure" to potential migration. We also hypothesized that the welfare benefit level in each state at the beginning of each period would affect changes in benefits during the period. If states are engaged in economic competition, we would be surprised if any state allowed its benefits to "get out of line." Just as states can be expected to cut benefits if they see that in-migration is occurring, they can likewise be expected to cut them if they fear benefits are high enough to encourage in-migration. Conversely, below-average states may cut their benefits less or even raise them, because they are less likely to become welfare policy havens.

Time period. Time period variables were included to control for the effects of changes in benefit levels occurring nationally. The national decline common to all the states needs to be isolated in the analysis in order to detect and explain relative changes in benefit levels among the states.

Determinants of Poverty Rates

A model representing the variables that determine changes in the residence of the poor is presented in Figure 1. We assume that personal economic situations affect both the likelihood of being in poverty and the probability that an individual will move outside the state. The decision to move is also affected by perceptions of prospects elsewhere, as well as by other preferences we are unable to measure directly. The sum of these economic effects and residential choices determines the change in state poverty rates.

Figure 1. A Model of the Factors Determining Changes in Poverty Rates


Economic Condition in Home State
Individuals' Economic Preferences
Individual Residential Decisions
Migration Rates
Individuals' Noneconomic Preferences

Notice that economic conditions have both short-term and longer-term effects on poverty rates. At any given moment economic conditions almost immediately influence a state's poverty rate by influencing the economic condition of individuals living within the state. The better the job opportunities, the higher the wage levels; and the lower the unemployment rate, the lower the state's poverty rate. But over time these same economic conditions may influence the distribution of poverty in quite the opposite direction by influencing locational decisions. Consider the following example. If wages within a state rise relative to other states, the immediate economic effect could well be to lower that state's poverty rate, as current residents become better off. But over time these higher wages (as compared to other states) may influence poor people's residential decisions: the poor within the state are more likely to stay, because they know earning opportunities are less elsewhere, while the poor in other states could well be induced by these higher wages to move into the state. All other things being equal, higher wages may eventually (and quite ironically) increase the state's poverty rate.

To separate the short-term and longer-term influences of economic conditions on a state's poverty rate, we have defined economic variables in two ways. We hypothesize that changes in economic conditions immediately influence changes in poverty rates: as a state's economy improves, poverty rates will immediately drop. We also hypothesize that economic levels influence poverty rates over the longer term by affecting residential choice: if a state's economy is performing at a high level, it will retain its poorer citizens and attract the poor from other states at a much higher rate than the nonpoor, increasing its poverty rate over time. In other words, we hypothesize that transportation costs are not prohibitively expensive and that the opportunity costs for a poor person are less than for someone who is not poor (Da Vanzo 1976; Gallaway, Gilbert and Smith 1967; Greenwood 1968). Note that these hypotheses predict that the coefficients for economic change and level variables will have different signs.

Changes in state poverty rates were estimated by the formula

$$\Delta P_{it} = b_0 + b_1(B_{ij}\Delta B_{it}) + b_2 W_{it} + b_3\Delta E_{it} + b_4\Delta I_{it} + b_5\Delta N_{it} + b_6 P_{2it} + b_7 P_{3it} + e_{it}$$

where $\Delta(P)$ = expected change in poverty levels; B = welfare benefit levels; ΔB = change in benefit levels; W = wage levels at beginning of time period; ΔE = change in the number employed; ΔI = change in per capita income; ΔN = change in population; P_2 = period 2 (1976–80)—dummy variable; P_3 = period 3 (1981–85)—dummy variable; i = lower 48 states and the District of Columbia, t_1 = 1971–75; t_2 = 1976–80; and t_3 = 1981–85.

Economic conditions. Change in employment (ΔE) and change in per capita income (ΔI) measure the impact of short-term economic influences on poverty rates. We hypothesized that as employment opportunities and per capita income increased, the immediate effect would be a decline in the poverty rate. Wage levels at the beginning of the time period was the indicator used to measure longer-term economic influences on the poverty rate. We hypothesized that poverty rates would increase where wage levels were high, because low-income people were especially likely to respond to higher earning opportunities in other states.[8]

Change in population. It is very difficult to identify a short list of variables indicating noneconomic reasons for residential choices that vary cross-sectionally and over time. To simplify matters, the change in the size of a state's population was included as a proxy for all the unmeasured variables—climate, "quality of life," and so on—that may systematically influence migration patterns. We hypothesized that—controlling for economic change and other factors—the states experiencing greater population growth would also have growing rates of poverty, because people with low income, having lower opportunity costs, would be more likely to move to more attractive areas.

Welfare benefits. Welfare benefits are an economic signal like any other. One need not think that welfare benefits are the *only* reason that the poor migrate to recognize that those in poverty—like others—are generally mindful of the economic prospects they face if they move. We expected higher benefit levels to attract low-income residents to the state in the same way higher wage rates did. We also hypothesized that increases in benefit level would have a smaller, but still positive, immediate effect on poverty rate changes. Furthermore, we hypothesized that poverty rates would be affected by benefit levels at the end of the period (equivalent to the interactive effect between the other two variables, with migration depending on both the initial level of benefits and whether they are going up or down). To examine this possibility, we estimated the effect of welfare benefits twice: The first time, we entered benefit levels and changes in benefits; the second time, we used only the interactive term.[9] Though the results from the first equation supported our hypotheses (Peterson and Rom 1988c, app. B), the estimation using only the expectations term produced larger and more significant coefficients.

National trends. Since our analysis combines data from three successive time periods, it is important to control for changes in poverty rates occurring nationally from one time period to the next. Over the 15-year period under investigation, national changes in poverty rates were substantial. The average state poverty rate fell from 14.6% in 1969 to 11.8% in 1975 before steadily rising to 13.5% in 1980 and 14.2% in 1985. The inclusion of the time period variables in the analysis enables us to isolate these changes from the interstate variation in poverty rate changes that are our focus.

The Determinants of Changes in Poverty Rates and Welfare Benefits

The results from four separate regressions for both the poverty and benefits equations are presented in Tables 2 and 3. The first two columns in each table include coefficients and summary statistics from the recursive equations using an ordinary least squares estimation. The third and fourth columns are from the simultaneous equations and are estimated by the two-stage least squares technique. Columns 1 and 3 are estimations from the raw data, while in columns 2 and 4 the data has been converted into logarithms. The results from all estimations are similar, but our interpretation will rely primarily on the simultaneous equations using the transformed data (col. 4).[10] Because the interpretation of the coefficients in the logged equations is complex (they show changes in proportional rates of change between the independent and dependent variables), we will describe the results in terms of the underlying variables. To make the effect of the coefficients on the variables more readily comparable within a range of conditions that states are likely to face, we have focused on how much the dependent variable would change if the independent variable shifted from one standard deviation below to one standard deviation above the mean.

The results indicate that policy makers do take into account the poverty rate—and the interstate migration of the poor—when establishing welfare policy. If poverty levels are 17.5% instead of 9% (comparing one state over time or two states at one time), annual welfare benefits in the high-poverty state for a four-person family would be $384 less. The change in benefit levels can be represented as follows:[11]

2-Standard-Deviation Increase in	Estimated Change in Annual Benefit Levels ($)
poverty rate	-384
initial benefit level	-1,212
tax effort	516
tax capacity	240
political competition	180

This effect of poverty on welfare policy can be interpreted in two different ways. One interpretation is that state policy makers have a more or less fixed budget for welfare expenditure, so that as the number of recipients increases, the alloca-

Table 2. Explaining the Change in Poverty

	Recursive Model		Simultaneous Model (2-Stage)	
Predictor	Unlogged	Logged	Unlogged	Logged
Benefits ($B\Delta B$)	.20 (1.4)	.12* (1.7)	.27* (1.7)	.17** (2.2)
Income (ΔI)	−.46 (1.6)	−.50* (1.8)	−.53* (1.8)	−.58** (2.0)
Employment (ΔE)	−.71* (1.9)	−.79** (2.2)	−.64* (1.7)	−.72* (1.9)
Wages (W)	.36*** (3.00)	.28*** (2.86)	.34*** (2.84)	.26*** (2.6)
Population (ΔN)	.51 (1.2)	.57 (1.4)	.42 (.9)	.48* (1.7)
Period 2 (P_2)	.36*** (8.9)	.36*** (10.0)	.36*** (8.7)	.36*** (10.0)
Period 3 (P_3)	.22*** (4.6)	.23*** (5.3)	.23*** (4.7)	.24*** (5.6)
Intercept	1.14*** (2.8)	−.28* (1.9)	1.20*** (2.9)	−.37** (2.3)
R^2	.54	.60	.55	.60
Standard error[a]	.160	.156	.159	.155
F-statistic	22.9	28.5	23.1	29.0
Number of cases	143	143	142	142

Note: t-statistics appear in parentheses.

[a] To make direct cross-equation goodness-of-fit comparisons possible the standard error for the logged equations was recalculated by taking the antilog of the predicted values and then calculating the squared difference between the actual and predicted values. This is the standard error reported here.

*$p < .10$
**$p < .05$
***$p < .01$

tion per recipient declines. Our estimates suggest that in the average state an increase of one standard deviation in the poverty rate would increase its welfare costs by $44 million per year by adding new recipients to the rolls. The state offsets this cost by reducing welfare benefits by an amount that saves the state $13 million. Since the savings that come from benefit reductions are less than one-third of the increased costs of assisting new recipients, the budget is clearly not on automatic pilot, equilibrating benefits with every change in the welfare rolls. Also, if it were simply a matter of keeping the welfare budget constant, welfare policies should be more sensitive to changes in the *number of welfare recipients* than to changes in the *poverty rates.* As the number of recipients increases, the level of support should drop accordingly. But the connection between changes in the number of welfare recipients and welfare policies proved to be insignificant. (See Peterson and Rom 1988c, app. B.)

That states seemed more sensitive to the overall size of the poverty population than to changes in the number of welfare recipients per se suggests the second interpretation, namely, that changes in the size of the poverty population are politically salient because they are visible to the gen-

Table 3. Explaining the Change in Cash and Food Stamp Benefits

	Recursive Model		Simultaneous Model (2-Stage)	
Predictor	Unlogged	Logged	Unlogged	Logged
Poverty ($P\Delta P$)	−.53***	−.08***	−.41***	−.08***
	(3.0)	(3.0)	(2.2)	(2.8)
Tax effort (T)	.18***	.21***	.17***	.20***
	(4.4)	(4.8)	(4.2)	(4.6)
Tax capacity (C)	.05	.10**	.06*	.09**
	(1.5)	(2.3)	(1.7)	(2.2)
Politics (PI)	.18**	.02**	.18**	.02**
	(2.3)	(2.3)	(2.3)	(2.2)
Benefits (B)	−.53***	−.39***	−.50***	−.40***
	(8.4)	(8.6)	(8.0)	(8.5)
Period 2 (P_2)	−.11***	−.12***	−.11***	−.11***
	(8.2)	(8.2)	(7.9)	(7.9)
Period (P_3)	−.10***	−.10***	−.09***	−.10***
	(6.6)	(6.6)	(6.1)	(6.3)
Intercept	1.16***	.62***	1.12***	.60***
	(15.7)	(6.2)	(15.2)	(5.9)
R^2	.53	.55	.52	.54
Standard error[a]	.065	.064	.064	.061
F-statistic	22.7	24.0	20.8	22.8
Number of cases	146	146	142	142

Note: t-statistics appear in parentheses.

[a] To make direct cross-equation goodness-of-fit comparisons possible the standard error for the logged equations was recalculated by taking the antilog of the predicted values and then calculating the squared difference between the actual and predicted values. This is the standard error reported here.

*$p < .10$.
**$p < .05$.
***$p < .01$.

eral public (see Plotnick and Winters 1985). As the number of the poor increases, so do the demands on a variety of social service delivery systems, including schools, hospitals, fire departments, and police. Politicians, interest groups, and the news media may ask where poor people are coming from and what is bringing them (Rom and Peterson 1988). The exact mechanisms connecting these events to public policy probably vary from state to state, but they seem to result from broader considerations than the budget alone.

Such reductions in welfare benefits are understandable, given the effect of benefit levels on the location of the poor. A state offering $722 per month in welfare benefits will have a poverty rate of 13.6% as compared to the 12.7% poverty in a state guaranteeing $476 per month. The change in poverty level can be represented as follows:[12]

2-Standard-Deviation Increase in	Estimated Change in Poverty Rates (%)
welfare benefits	.9
per capita income	-1.0
wages	1.0
employment	-1.7
population size	.8

Wages have an effect on poverty rates

similar to that of benefit levels. If a state with a below-average hourly wage of $7.94 has a poverty rate of 12.7%, a state where wages average $10.52 will have a poverty rate of 13.7%. Rapid population growth also contributes to increases of poverty: a state with a 13.1% increase in population over a five-year period will have a poverty rate of 13.6%, compared to a 12.8% poverty rate for a state that shrank by .3%.

While states with high welfare benefits, hourly wages, and population growth have quite similar long-term increases in their poverty rates, states with rapidly growing economies enjoy immediately shrinking poverty rates. Rising per capita income depresses poverty: a state with 15.5% income growth (as compared to 1.3%) over five years will experience a reduction in its poverty rate from 13.7% to 12.7%, while an 8.4% increase in income leaves the poverty rate unchanged. More importantly, a 24% increase in employment over five years reduces poverty to 12.4%, while a 3.3% gain in employment would leave poverty at 14.1%.[13] As might be expected in a society in which income is not equally distributed, growth in the number of jobs reduces poverty faster than growth in per capita income. Indeed changes in employment opportunities affect the poverty rate substantially more than any other single variable.

Although none of these factors individually has a dramatic impact on the distribution of poverty in the United States, they are not insignificant. In an average-sized state a 1% increase in the poverty rate adds about 55 thousand to the rolls of the poor. And except for changes in employment, welfare benefits have about as strong an effect on the location of the poor as any of the other variables.

Benefit levels are the only identifiable factor influencing poverty rates that state policy makers directly control, and there is considerable evidence that state politicians take the effect of poverty increases on the state's economy into account. Not only do states with rising poverty reduce their benefits, but states with high benefits relative to other states—fearful, we suspect, of becoming welfare havens—cut them. This may be understood as a powerful convergence factor that shapes policy outcomes. If a state's benefits are higher than its peers', policy makers are increasingly pressured to adjust their benefits downward. Given these competitive pressures from other states, one might expect that policies among the states would eventually converge to a single national standard, the result of mutual policy adjustment among the states.

Two things must be understood about this convergence, however. First, convergence does not happen all that quickly. In a computer simulation in which initial benefit levels were the only explanatory variable and with benefits spread from a low of $476 per month to a high of $722 per month (one standard deviation above and below the mean for our data set), after four periods (20 years) the most generous state had lowered its benefits to $624 and the least had raised its to $570. Second, the convergence can occur around a steadily falling average. The point at which benefits converge depends on how the average state reacts. If a state offering the average level of welfare does not cut its benefits, benefit levels will converge at approximately this average. But if states at the mean, perceiving themselves nevertheless as too generous, cut their benefits, the average benefit level will spiral downward.[14] This, in fact, appears to have happened, for welfare benefits have steadily declined since 1970.

The pressures toward convergence are also offset by opposing political and economic factors. The most important of these seems to be the political culture of the state as measured by a state's willingness to tax itself. A state with a high tax effort increases its benefits by $42 a month as compared to a state with a low tax effort. Greater political competitiveness and state wealth also contribute to rising benefits: a highly competitive state is predicted to increase benefits by $15 a month more than one with weak political competition, and a state with high tax capacity should increase benefits by $20 a month over a state having low tax capacity.

These results suggest that state policy makers take into account multiple signals in determining welfare benefit guarantees. States that are more prosperous, more politically competitive, and culturally more disposed toward redistribution are likely to increase welfare benefits over time and in comparison to other states. But high and increasing poverty levels lead to cuts in welfare. High benefit levels also expose states to welfare-induced migration of the poor. During the seventies and eighties the conditions that promote higher benefits were outweighed by forces that prompt benefit cuts.

Conclusions

Our research is consistent with theoretical analyses showing that state and local governments in a federal system will tend to provide less redistribution than if policies were set nationally. Each state government acts as if it prefers the provision of welfare services to be made by other state governments and fears to provide full services lest it become attractive to poor people. Since every state makes a similar calculation, redistribution levels do not reach the level that would probably be set by the national government.

State redistributive policies are below this level in part because low-income people are sensitive to interstate differences in welfare policy. This does not mean that large numbers of poor people rush from one state to another with every modest adjustment in state benefit levels. But the data do suggest that over time, as people make major decisions as to whether they should move or remain where they are, they take into account the amount of welfare provision a state provides and the extent to which it is increasing. The poor do this roughly to the same extent that they move in response to differences in wage opportunities in other states.

Because of the pressures on the states to cut benefits, the equilibrium toward which the decentralized system tends is probably below the equilibrium that would obtain if policy were uniformly set by the national government. The best evidence for this is the differential trend in food stamp and cash maintenance policies over the past 15 years. During this period of time food stamp benefit levels, which are determined and funded entirely by the federal government, have survived strong fiscal pressures during both Democratic and Republican administrations. Indeed, the average food stamp benefit paid to a person receiving cash assistance actually increased by 31% between 1970 and 1985. In addition, food stamp coverage was extended from 53% to 83% of the population receiving cash assistance (U.S. House of Representatives, Committee on Ways and Means 1986, 393). During this same period of time the level of cash benefits, which is determined by state governments, fell by an average of 35%.

Theoretically, the states and the federal government have the same political constituency. But in practice the political pressures with respect to redistribution have manifested themselves in two quite different structural contexts. At the state level public officials must take into account the effects of their policies on increasingly mobile labor and capital markets. Providing higher benefit levels could prove to be costly for the state economy and thus politically unattractive to state politicians. At the federal level interest groups specializing in welfare policy concentrate their attention on the committees responsible for the policy arena, which tend to attract members with distinctive policy commitments. As both parties seek to avoid being accused of cutting holes in the safety net for the poor, the pressure to cut benefits is resisted even when overall fiscal constraints may seem at least as restrictive nationally as locally (Peterson and Rom 1988b).

It may be argued that welfare policy should vary from one part of the country to another so that people with differing ideas about the desirable level of redistribution can live in a state whose practices conform to their own beliefs (Pauly 1973). There is some evidence in our data to support this argument. In states with a moralistic political culture as measured by a state's tax effort, welfare benefits tend to increase relative to other states. Also, in states that have higher levels of political competition and electoral participation, welfare benefits increase. But if these findings provide some support for this argument, other findings give as much or more support to the counterargument that welfare benefits for the poor should not depend on the accidents of a state's wealth, as measured, for example, by a state's tax capacity or the size of its low-income population.

Welfare policy in the United States has already begun to take these economic realities and equity considerations into account. As the nation's economy has become increasingly integrated during the postwar period, the fiscal responsibility for welfare has steadily shifted to the national government. In 1940 the federal government paid for only 38% of the cost of the cash maintenance program. By 1985 the federal share had increased to 77% (food stamp contributions being taken into account; Peterson and Rom 1988a). If a national welfare standard were adopted, it would simply further a trend that is already well established.

Notes

1. In a 1974 review of this literature, Long concluded that "no study has presented empirical evidence for the hypothesis that welfare payments themselves have attracted large numbers of persons to states or cities with high benefit levels. Most factual analyses of welfare have considered the hypothesis and refuted it, but the evidence presented has not been entirely convincing" (p. 48). He draws similar negative conclusions from his own research. In a second review Holmer (1975, 33) concludes, "Differences in state AFDC policies have, at most, a minor influence on the residential location decisions of the poor." For an extensive list of studies considering the impact of welfare on migration, see Peterson and Rom 1988c.

2. Blank's (1985) study shows that the combination of differential wages and welfare benefits induces migration from one region to another. However, her study does not disentangle the independent effects of wages and benefits, combines the 50 states into 10 regions (thereby reducing the precision with which the welfare benefit variable is measured), and does not take into account the possibility of simultaneous effects of benefits and migration on each other.

Gramlich and Laren (1984), in addition to supporting Blank's individual-level findings, develop a model using aggregate data to estimate the simultaneous relationship between welfare benefits and the recipient population. They find that the effect of welfare benefits on migration is strong and significant. Their analysis of the determinants of benefit levels is underspecified, however, inasmuch as it includes only two economic factors: the price of AFDC benefits and per capita income.

3. All benefit levels we reported are calculated in constant dollars, using 1985 as the base year.

4. Direct comparisons are not possible, because maximum welfare benefit levels are not available for 1960 and average benefits (available for 1960) are not available for the combined food-stamp-and-cash programs in 1985. However, the recipient in the average three-person family receiving the cash benefit of the average state ($292) would be entitled to $415 in combined food-stamp-and-cash benefits, as compared to an average state's median cash payment of $413 in 1960.

5. Authors' calculations based on data from Ross and Danziger 1987.

6. When we could not obtain data for exactly the same year, we used data from the closest possible year, which, in all except two instances, was within a year. See Peterson and Rom 1988c (app. A) for details.

7. Change is defined throughout as a ratio in which Y_{j+1}/Y_j, where j is the beginning of the time period and $j + 1$ is the end of the time period.

8. Initially we included in our analysis seven economic variables: change in per capita income, change in wages, change in unemployment rate, change in number of jobs, per capita income, wage levels, and unemployment rate. We retained the two that were individually significant and provided for the best overall fit.

9. Because of a very high degree of multicollinearity, entering all three terms in one equation would produce unreliable results. See Lewis-Beck 1980, p. 56.

10. Preference for logged or unlogged data depends on expectations about marginal relationships, precision of the coefficients, and overall goodness of fit for the model (Hanushek and Jackson 1977, 100–101). There is no reason to believe that there is a linear relationship between the variables; the marginal relationships from the logged equations are quite consistent with our expectations, as will be discussed. The transformed data appears to yield superior estimates by the last two criteria: they provide more precise estimates (judging by the size of the *t*-statistics) and smaller standard errors for the entire model. Two-stage least squares estimation provides superior (consistent) estimates over ordinary least squares in nonrecursive equations.

11. The estimations were made by setting to zero the effects of the mean values of all explanatory variables on benefits and then estimating the effect of a change from one standard deviation below the mean to one standard deviation above the mean for each explanatory variable.

12. See n. 11.

13. Again, these economic growth rates represent rates one standard deviation above and below the mean for our data. Roughly speaking, states were as likely to differ by 14.2% in per capita income growth as by 20.6% in employment growth.

14. This is a matter of changing the intercept term. In our simulations with the average state not cutting its benefits, its intercept (for the logged estimation) is calculated with the equation, intercept $= 1/(B)^{-.40}$, where B = average benefits and $^{-.40}$ is the benefits coefficient from our empirical estimations (Table 3). Reducing the size of the intercept means that the average state will cut its benefits. To show this we used intercept $= .9/(B)^{-.40}$. By doing this, the average benefit level in our simulation dropped from $599 per month to $486 after four periods.

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POLITICAL PARTIES, PUBLIC OPINION, AND STATE POLICY IN THE UNITED STATES

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When comparing states in the United States, one finds little correlation between state opinion and party control of the state legislature or between party control and state policy. Although these low correlations seeming to indicate that partisan politics is irrelevant to the representation process, the opposite is true. State opinion influences the ideological positions of state parties, and parties' responsiveness to state opinion helps to determine their electoral success. Moreover, parties move toward the center once in office. For these reasons, state electoral politics is largely responsible for the correlation between state opinion and state policy.

One central question about modern democracies is, How much do partisan elections contribute to the effective representation of public opinion? In theory, programmatic parties provide the electorate with a clear choice of policy options, and election results are followed by predictable policy consequences. But empirically, does it matter which party (or coalition) governs? Most recent cross-national comparisons of Western democracies answer affirmatively. Socialist or leftist party participation in the national government is associated with a high inflation-to-unemployment trade-off (Hibbs 1977), growth of the governmental sector (Cameron 1978; Lange and Garrett 1985), high public expenditure (Tufte 1978; Castles 1982), welfare spending (Castles 1982; Hicks and Swank 1984), quality-of-life (Moon and Dixon 1985), and income equality–redistribution (Tufte 1978; van Arnhem, Corina and Schotsman 1982; but see also Jackman 1975, 1980). In sum, nations where leftist parties are most influential also tend to have the kinds of policies and policy consequences with the greatest appeal to leftist constituencies. Time series analyses for individual nations also show policy consequences resulting from party control (Alt 1985; Hibbs 1977, 1987). Party control does matter.

It may be the case, however, that party control matters only in terms of the sharp divisions between European Socialist parties of the Left and parties of the Right. In the U.S. context, it is by no means clear that major policy differences flow from variation in Democratic versus Republican control. At the national level, while some time series find policy consequences from party control of the presidency or Congress (Hibbs 1977, 1987; Kiewiet and McCubbins 1985), the role of parties re-

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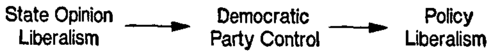
mains in some dispute (Beck 1982; Browning 1985; Lowery 1985). At the state level (our focus here) the policy relevance of party control has been viewed with particular skepticism.

In state policy research, the apparent irrelevance of political parties has been a persistent puzzle. Presumably, Republican states would have the most conservative policies and Democratic states the most liberal policies. But this is not the case. The relative strengths of the Republican and Democratic parties in state politics appear to be statistically unrelated to policy directions in the states even after the imposition of rigorous controls (Dye 1966; Plotnick and Winters 1985; Winters 1976). Only time series analyses show any evidence of the expected party effects, with welfare spending responsive to party control in some states but perhaps not in others (Dye 1984; Garand 1985; Jennings 1979).

The nonrelationship between party control and state policy appears to deny common, seemingly informed, stereotypes of how politics in the United States works. Most observers would agree that Democratic and Republican elites differ ideologically in their values and in certain manifestations of behavior, such as roll call votes. Could this be an illusion? Perhaps in state politics, the two parties have become indistinguishable on policy substance—just what spatial models of party competition predict should happen for electorally driven parties (Downs 1957).

Or perhaps the seeming irrelevance of parties is confirmation of the general theory that policy outcomes are determined by environmental, rather than political, variables (Dye 1966, 1979). Indeed, the failure of party control to predict policy may be the strongest evidence available in support of the environmental theory. Although Republican and Democratic candidates diverge in their campaign rhetoric and even in their personal values, perhaps Republican and Democratic politicians ultimately enact the same policies when given the responsibility of governing. By this scenario, politicians in office respond to the social needs and economic constraints of the state at the expense of campaign pledges or other ephemeral political considerations.

Figure 1. A Simple Model of State Public Opinion and Policy



A Broken Electoral Linkage?

Does electoral politics in the states really not matter? Recently we reported evidence that challenges the economic determinism interpretation of state politics (Wright, Erikson, and McIver 1987). We discovered that despite the apparent strength of economic variables as predictors of state policies, state public opinion is strongly correlated (r = .80) with state policies and is a much stronger predictor than economic variables considered individually or as a group.

Clearly, the ideological preferences of state electorates play an important role in determining the ideological tone of state policies. Partisan elections would seem a likely conduit for this flow of causality, with liberal states electing Democrats who enact liberal legislation and conservative states electing Republicans who enact conservative legislation. This simple model is diagramed in Figure 1.

But our data do not fit this simple model.[1] Echoing earlier findings, we find Democratic party control of state legislatures unrelated (r = −.22) to an eight-item measure of state policy liberalism. Moreover, even if party control could account for policy, the linkage between opinion to policy via parties would be broken at the front end. The correlation between the liberalism of state publics and Democratic control is only .07. Thus, liberal and conservative states do not differ in their tendencies to elect Republicans or Democrats to state office. The strong effect of state opinion on state policy would seem to bypass the electoral process.

Not only are the ideological preferences of state electorates uncorrelated with the partisan composition of state elected officials but they are also unrelated to the electorate's partisan preferences. The mean liberalism of state public opinion correlates only .01 with mean Democratic party identification. State party identification, however does correlate with party control ($r = .86$), as one would expect.

Democratic states do elect the most Democrats to state office. But the most Democratic state electorates are not unusually liberal. And the Democratic legislatures they elect do not enact unusually liberal legislation. All this would seem to deny the presence of much policy representation. But we must remember that the most liberal state electorates do elect legislators who enact the most liberal legislation, even with the absence of any obvious partisan linkage.

Are political parties as irrelevant to the representation process as this analysis suggests? Perhaps political parties do play an important role, but the linkage is more complicated than has been presented so far. But to pursue this possibility, one needs a theory of how partisan elections fit into the representation process in the states.

A Theory of State Electoral Politics

A theory of state electoral politics must take into account the wide variation in the ideological orientations of state political parties. To take an obvious example, the Democratic party of Mississippi is far more conservative than the Democratic party of New York and perhaps than the New York Republican party as well. With the Democratic and the Republican parties each differing ideologically from one state to the next, the potential exists for serious confounding of our understanding of how parties contribute to the policy representation in the states. This ideological variation of state parties sometimes appears to be chaotic and idiosyncratic to particular state political cultures. But there is good reason to suspect some order and predictability in both the source of the variation and its electoral consequences.

To develop some propositions about state party ideology, party electoral strength, and state policy, let us take the role of elections in the policy process seriously. Suppose that state electorates actually reward and punish the parties on the basis of their policy positions and specifically on the basis of their placement of the left-right continuum; that is, suppose that state electorates tend to reward the party that best reflects their ideological viewpoints. Suppose also that state parties are motivated (at least in part) to win elections. Then, to the extent that parties are electorally motivated, they should respond to state opinion. To the extent that parties respond effectively to state opinion, they should achieve electoral success.

When state parties respond to state opinion, the most conservative parties would be found in the most conservative states and the most liberal parties would be found in the most liberal states. Of course, we do not expect state parties' ideologies to be driven *entirely* by state opinion. If they were, state parties would always converge to the position of the state's median voter. In actuality state parties, like national parties, are pushed toward the median voter position by electoral considerations and away from the median voter position by the preferences of their activists (Aldrich 1983; Calvert

1985; Poole and Rosenthal 1984; Wittman 1983).

To the extent that state elections are decided by ideology, state electoral success should go to the party that is ideologically closest to the state electorate. Thus, Republican state parties should be most successful when they move toward the liberalism of the Democratic opposition. Democratic state parties should be most successful when they move toward the conservatism of the Republican opposition. These policies could backfire, but only if the parties move too far, as when the Democrats get so conservative that the Republicans outflank them on the left. But this does not happen with state parties.

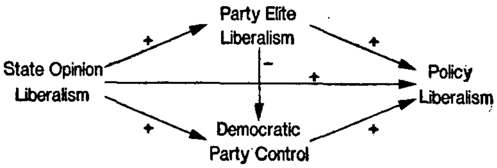
To summarize, it is the ideological flexibility of the Democratic and Republican parties in the states that obscures the linkage between state party control and public policy. But this is not to say that the partisan electoral process in the states is unimportant. As we will show, ideologically flexible parties can respond to state public opinion and in turn, be electorally rewarded and punished based on how well they represent state opinion.[2]

Our argument can be recognized as a variation of the familiar Downsian theme that electoral rewards lie in the political center (Downs 1957). Appendix A explicates the formal argument in greater detail and illustrates propositions with hypothetical examples. We now explore the implications for estimating the role of party control in the representation process in the states.

A Model of Electoral Representation

According to our model, centrist parties are electorally rewarded. This means that the Democrats are rewarded for movement in the conservative direction and the Republicans for movement in the liberal direction.[3] In short, liberal parties (Democratic or Republican) negatively affect Democratic success. Figure 2 introduces party positions to the model of state electoral representation. We label this variable party elite liberalism. Note how electorally responsive parties can account for the absence of correlations between opinion liberalism and Democratic success and between Democratic success and policy liberalism.

Figure 2. Representation Model with Party Elites



In our model, liberal opinion causes liberal parties. However, when state electorates respond to party positions, liberal parties hinder Democratic success. This makes a negative indirect path from opinion liberalism to Democratic control. At the same time, the model shows a positive direct effect of opinion liberalism on Democratic legislative strength. The net effect of the combined indirect and direct paths could well be a low-bivariate correlation between state opinion and party control.

The model also accounts for the low correlation between party control and policy. According to the model, party ideology suppresses the positive relationship between party control and policy; liberal parties work against Democratic control but for liberal policies. This source of spurious negative correlation obscures the posited positive effect of Democratic control on liberal policy.

Figure 2 also shows how parties' responsiveness to state opinion contributes to representation in a rather straightforward fashion. Liberal state opinion causes liberal state party elites, which results in liberal policies. Finally, the model allows for the direct effect of opinion on policy independent of partisan variables.

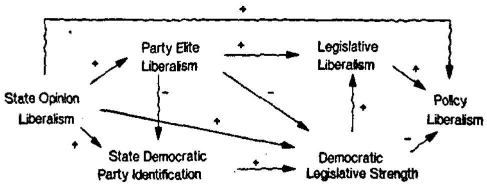
The model can be complicated further. Figure 3 adds two more variables: state party identification and the liberalism of the state legislature. Figure 3 also adds specificity to the meaning of party control by redefining it in terms of Democratic legislative strength.

This new model places party identification as an intervening variable between opinion liberalism and Democratic legislative strength. Partisanship provides a mechanism for translating public opinion into legislative power: liberal states tend to identify with the Democratic party, which increases the number of Democratic state legislators. Surprisingly, as we reported, the correlation between opinion liberalism and Democratic party identification in the states is quite weak. But the role of party elites can explain this anomaly: liberal states cause liberal parties, which over time contribute negatively to Democratic partisanship. This effect hides the expected relationship between public opinion and partisanship in which liberals tend to identify with the Democratic party.

The liberalism of the legislature is introduced as an intervening variable caused by the degree of liberalism among the two major parties' elites and the partisan division of the legislature: the more liberal the parties and the more Democrats in the legislature, the more liberal the legislature should be. The inclusion of legislative liberalism provides an additional subtlety to the model. Electing Democrats should liberalize the legislature, which leads to liberal policies. But as Figure 3 suggests, with legislative liberalism and public liberalism held constant, the residual direct effect on policy liberalism of electing Democrats could be negative.

The ceteris paribus condition is crucial for this proposition. We do not seriously speculate that electing Republicans is the way to get liberal policies (although speculation of that sort can be found in the literature: see Goodin 1983). Rather, under identical political circumstances and with identical ideological values, Republican and Democratic legislators should respond in different directions to electoral motivations: Republicans should find more votes by moving left and Democrats should find more votes by moving right. Of course Republican and Democratic legislators tend to differ in their ideological values. But conceivably, electoral motivations could be so strong that conservative Republicans and liberal Democrats make ideologically similar decisions when in control of the state legislatures.

Figure 3. Full Representation Model



In summary, the model of Figure 3 shows how state opinion can influence state policy via a variety of indirect electoral pathways. These pathways are based on the assumptions that state parties respond to state opinion and that state electorates respond to party positions. If this theoretical development is correct, electoral politics can be crucial for policy representation in the states, even though party control is statistically uncorrelated with both state opinion and state policy.

One important omission from this model is the governor's role in state policy. This omission is due largely to measurement considerations rather than to disregard for the governor's role in the legislative process. When the dependent variable is a set of policies cumulated from years of legislative decision making, it makes little sense to include the governor's party affiliation for the short-term as an independent variable. And partisan control of governorships over the long term bears little relationship to other indicators of partisanship. Where unusual long-term success for a party is found at the gubernatorial level (e.g., the Democratic in Utah, the Republican in Michigan), this may result precisely because of gubernatorial leadership that is ideologically atypical for the party. We are far more reluctant to assume that governors share the ideological positions measured for their party's elite than we are that, on the average, state legislators do so.

While we do not include the state's chief executive in our model for measurement reasons, this decision should not significantly distort the model we present. Much has been made in recent years of the increasing formal authority of governors. Yet little evidence exists that demonstrates an empirical link between the powers of the executive and actual policy direction. For instance, state representatives rarely see the governor as playing a significant role in the state legislature. Legislators rank the governor last among thirteen potential decision cue sources: less than 2% of all state legislators choose the governor as the principal informant for their decisions (Uslaner and Weber 1977, 34).

Measurement

To put the theory to use, it is necessary to operationalize each of the component parts. Most critical is the measurement of the one "brand-new" variable: party elite liberalism. In all, we were able to assemble four separate indicators of the ideological preferences of Democratic and Republican state elites, which we combined into a comprehensive measure of elite opinion. The four indicators are

1. *Congressional candidates' conservatism-liberalism.* Using the 1974, 1978, and 1982 CBS-*New York Times* surveys of congressional candidates, we ascertained the ideological position of each congressional candidate who ran in these three election years. First, the issue responses to these surveys were summed to create ideological scores (Erikson and Wright 1989; Wright 1986; Wright and Berkman 1986). Second, ideological scores were standardized across the election years using the scores of the incumbents who served throughout the period. Third, means were calculated by party and state. The result is the measurement of the mean ideological position for each party's candidates in each state over three elections.

2. *State legislators' conservatism-liberalism.* For the more than a thousand state legislators surveyed by Uslaner and Weber in 1974 for their state legislative decision-making study (Uslaner and Weber 1977), we constructed factor scores to measure their ideological positions. These were calculated at the individual level using the legislator's stands on the death penalty, abortion, pollution regulation, gun control, teachers' unions, and legalization of marijuana. Factor scores were averaged for the legislators of each state party.

3. *Local party chairmen's conservatism-liberalism.* As part of their study of state and local party organizations, Cotter and his colleagues ascertained the ideological identifications of county party chairs in 1979–80 (Cotter et al. 1984). We use the mean ideological position of Republican and Democratic county chairs in each state as a third measure of party elite liberalism.

4. *National convention delegates' conservatism-liberalism.* Ideological self-identification of delegates to the Democratic and Republican national conventions in 1972, 1976, and 1980 has been collected by Miller and Jennings (1987; also see Farah, Jennings, and Miller 1981). Here we use the mean ideological identifi-

cations of the state party delegations over these three convention years.

For many analytic purposes, these four indicators could be treated separately, as distinct measures of different phenomena. Here, we are interested in the general ideological positions of state party elites, especially as they might be viewed by the attentive electorate. For this purpose, we combine the four indexes. Each of the four is an imperfect measure of party positions. By combining them into one composite index, we strengthen the instrument as a measure of party elite position.

These eight (four sets for two parties) sets of ideological scores correlate strongly with each other. We could choose any of several methods for combining these scores in a grand index, and the exact procedure makes little difference. In the present analysis, we used the following procedure: Each set of scores was standardized over the 94 separate parties (for 46 states, with Alaska, Hawaii, Nebraska, and Nevada excluded). The four sets of Republican elite scores and the four sets of Democratic elite scores were then summed to produce composite Republican and Democratic indices. For much of the analysis we simplify further by employing the average (or midpoint) ideological position of the two state party elites.[4]

Operationalization of other variables can be described more briefly: The liberalism of *state opinion* is measured as the mean ideological identification (conservative-moderate-liberal) in the pooled CBS-*New York Times* surveys over the 1976–82 period. These state estimates, some based on pooled state samples larger than the typical national survey, are reliable at .82 or above (Wright, Erikson, and McIver 1985). *State party identification* is measured as the mean party identification (Republican-Independent-Democrat) in the CBS-*New York Times* pooled samples (Wright, Erikson, and McIver 1985).

Democratic state legislative strength is measured as the average of the Democratic strength for each legislative chamber over the 1977–84 period. Upper and lower chambers are weighted equally. *Legislative liberalism* is measured indirectly as the weighted average of the Democratic and Republican elite liberalism scores, where the weights are determined by the parties' relative legislative strength (weighing the two chambers equally). In other words, we assume that the average liberalism of a state's Democratic legislators is the mean composite liberalism score for the state's Democratic elite; and we assume that the average liberalism of a state's Republican legislators is the mean composite liberalism score for the state's Republican elite. Democratic and Republican composite liberalism scores are then combined in proportion to the two parties' average legislative strength.

State policy liberalism is the standardized sum of state policy scores on eight separate issues: education, Aid for Families with Dependent Children, criminal justice, gambling, consumer protection, progressive taxation, and Equal Rights Amendment ratification. These items form one common dimension, with an estimated reliability (alpha) of .89 for the composite measure (Wright, Erikson, and McIver 1987).

The following sections report on the empirical fit of the causal model presented above in Figure 3. For handy reference for these sections, Table 1 reports the correlation matrix for the variables; Table 2 shows the results of the standardized regression equations; Figure 4 shows the

Figure 4. Path Model of the State Policy Process (46 States)

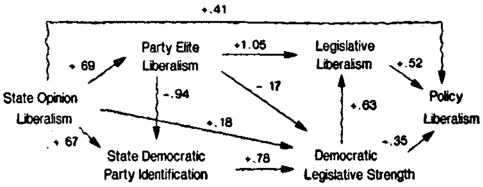


Table 1. Correlations among Variables for Model of State Policy Process

Variables	Opinion Liberalism	Party Elite Liberalism	Democratic Party Identification	Democratic Legislative Strength	Legislative Liberalism	Policy Liberalism
Opinion liberalism		.69	.01	.07	.79	.80
Party elite liberalism			−.48	−.42	.79	.85
Democratic party identification				.86	.01	−.33
Democratic legislative strength					.19	−.22
Legislative liberalism						.78
Policy liberalism						

Note: Alaska, Hawaii, Nebraska, Nevada are excluded.

causal model with the estimated standardized path coefficients (betas) included.[5] A case can also be made for doing the analysis without the potentially confounding (and interactive) effects of southern regionalism. Accordingly, we present a brief parallel analysis in Appendix B, with the southern states removed.

Public Opinion and Party Elites

First, we consider the relationships between state opinion and the positions of the Democratic and Republican party elites. The correlations are .54 for Democratic elites and .72 for Republican elites. The average of the two party positions (or the midpoint) correlates at .69 with state opinion.

These correlations suggest a healthy responsiveness by parties to opinion. Part of this responsiveness is merely that state party elites reflect to some extent the ideology of the general publics from which they are drawn (the "sharing" model: Erikson, Luttberg, and Tedin

Table 2. Standardized Regressions for Path Model of Policy Process

	Dependent Variable				
Independent Variables	Democratic Elite Liberalism	Democratic Party Identification	Democratic Legislative Strength	Legislative Liberalism	Policy Liberalism
Opinion liberalism	.69* (.11)	.66* (.17)	.18 (.13)		.41* (.11)
Democratic elite liberalism		−.92* (.17)	−.17 (.14)	1.05* (.04)	
Democratic party identification			.78* (.10)		
Democratic legislative strength				.63* (.04)	−.35* (.07)
Legislative liberalism					.52* (.11)
Adjusted R^2	.47	.46	.74	.95	.81

Note: Alaska, Hawaii, Nebraska, Nevada are excluded. Standard errors are in parentheses.

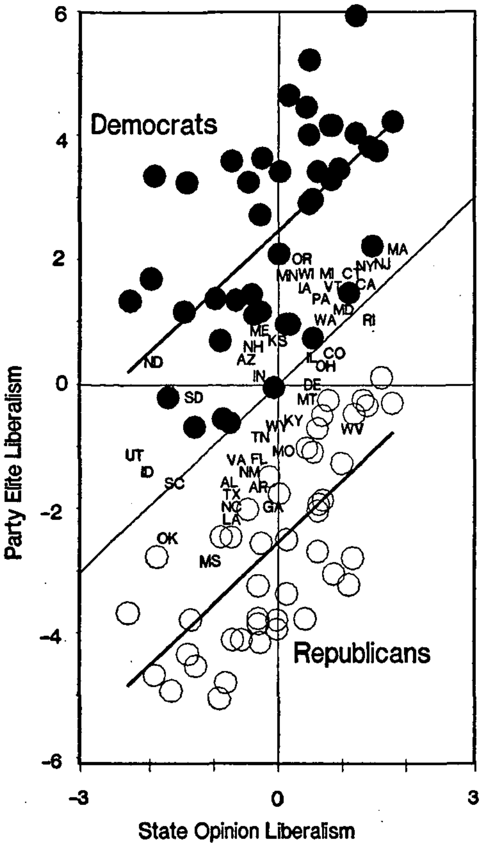
$^{*}p < .001$, two-tailed test.

1988). A second contributing factor—and for electoral politics a more interesting one—is that state parties deliberately modify their positions in response to state opinion, from fear of electoral sanctions.

But while state parties respond to public opinion, they certainly are not the centrist electoral machines of Anthony Downs's model. State parties clearly represent the ideological predilections of their activists. Figure 5 shows how the state parties vary as a function of state opinion but remain ideologically distinct from one another. The unusual dimensions of this figure represents an attempt to scale state parties and state opinion on a common metric. The horizontal axis of this scatterplot is the liberalism of state opinion, scaled as a standardized measure. The vertical axis represents the liberalism of party elites. The scale for the vertical axis is set so that the expected midpoint between the two party positions equals state opinion. In other words, this scaling procedure assumes that on average neither party is more representative than the other and that the relative representativeness of the two parties is unrelated to state opinion.

Figure 5 illustrates how state parties position themselves relative to state opinion and their partisan opposition. In every state, the Democratic elite is more liberal than the Republican elite. And given the scaling assumptions, each Democratic party is more liberal than state opinion, and each Republican party is more conservative than state opinion. In most states, the divergence of the two parties is considerable: typically, the divergence between the state's Democratic and Republican positions exceeds the entire range of mean state opinion between the most conservative and the most liberal states.

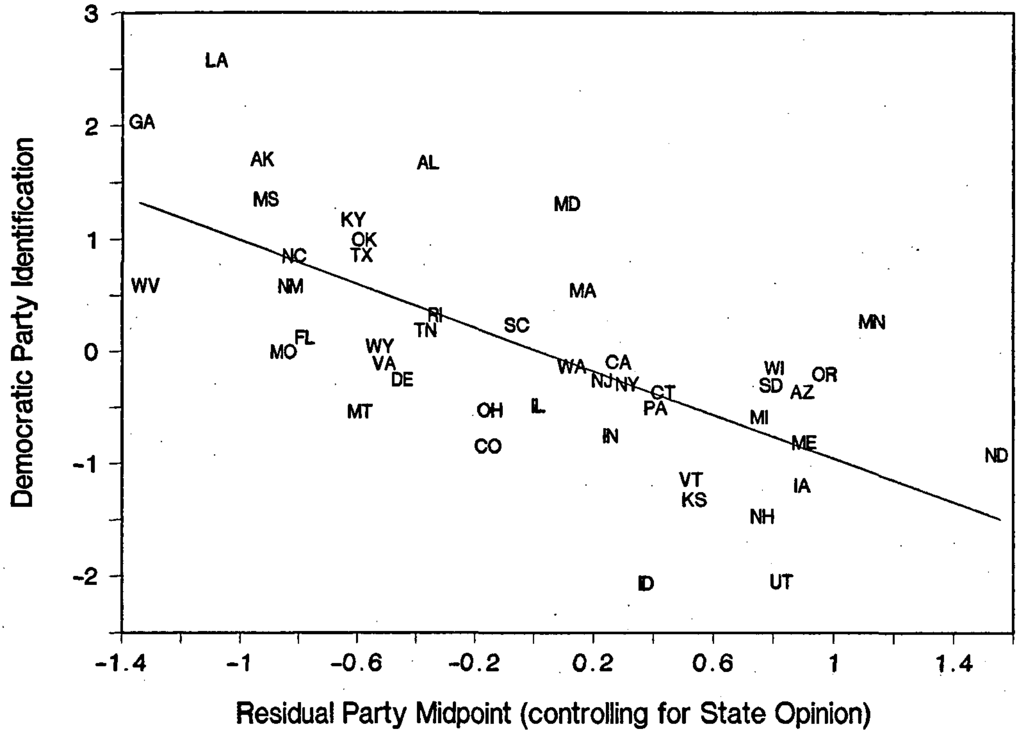
Figure 5. Party Elite Liberalism by State Opinion



Note. Solid dots represent Democratic elites; hollow dots represent Republican elites. State labels are placed at the midpoint between the Democratic and Republican elite positions.

Clearly, Figure 5 does not present a picture of ideological convergence, with political parties mimicking the ideological stance of the state's median voter. The considerable within-state divergence of the two parties' ideological positions suggests a strong ideological pull by the parties' activists. But this tendency toward ideological extremism is not reason to ignore the responsiveness of the parties to state opinion. Figure 5 shows how state party positions vary in response to state opinion: the more liberal the state, the more liberal the Democratic party and the more liberal the Republican party. On the one hand activists push state parties away

Figure 6. Party Identification by Party Elite Liberalism



from the center; on the other hand, electoral considerations push state parties back toward the center.

State Opinion, State Party Positions, and Party Strength

We argued that party positions should influence state partisanship, with liberal parties hurting the Democrats. We see in Figure 4 that the estimated path coefficient confirms our theoretical expectation. The liberalism of party positions (summarized as the party midpoint) shows a quite dramatic negative effect on Democratic party identification (beta = −.94). The control for party positions also confirms another important theoretical expectation: the initial opinion-party identification correlation was only .01; but now, with party positions controlled, we find the expected positive influence of state opinion on state party identification (beta = .67).

The responsiveness of state party identification to party elite ideology is shown in Figure 6. This figure shows the scatterplot of the relationship between state party positions, measured as the relative deviation of the party midpoint from the midpoint predicted from state opinion, and state party identification. Clearly, parties that are liberal relative to state public opinion push state electorates toward the Republicans, and parties that are conservative relative to state public opinion push states toward the Democrats.

It may seem dissonant to see state electorates respond to ideological positions of state parties in terms of party identification. This finding conflicts with two supposed axioms of U.S. politics—that party identification is unmoving and that state politics is largely invisible to voters. The so-clearly observed pattern, however, does not dictate that state electorates frequently change their partisan character in response to short-term ideological maneuvering by state parties. Much more plausibly, it represents the influence of state party positions over the long haul, with the long-term ideological tendencies of state parties accumulating a long-term influence on the party identification of the state's electorate.

State party identification takes on importance because it is the most important direct cause of state legislative partisanship (beta = .78). Thus, via their influence on state party identification, state opinion and state party positions indirectly influence legislative partisanship. Direct effects are also present. Although the coefficients are not statistically significant, state opinion appears to influence legislative partisanship positively (beta = .18) while the ideologies of state party elites appear to influence legislative partisanship negatively (beta = −.17), as predicted. In other words, the relative ideological positions of state opinion and party positions may influence legislative elections directly, even beyond the predominant paths via party identification. This may represent aggregate evidence of "issue voting" in state legislative elections beyond the long-term component channeled through state party identification.

We see now how public opinion influences legislative party strength even though state opinion does not correlate strongly with legislative party strength as the simplest model would predict. Liberal electorates do cause Democratic legislatures; but liberal state electorates also result in liberal state parties, which works to the electoral advantage of the Republicans. Both processes are surprisingly strong statistically when the appropriate controls are imposed. But they are not ordinarily visible because they cancel each other out.

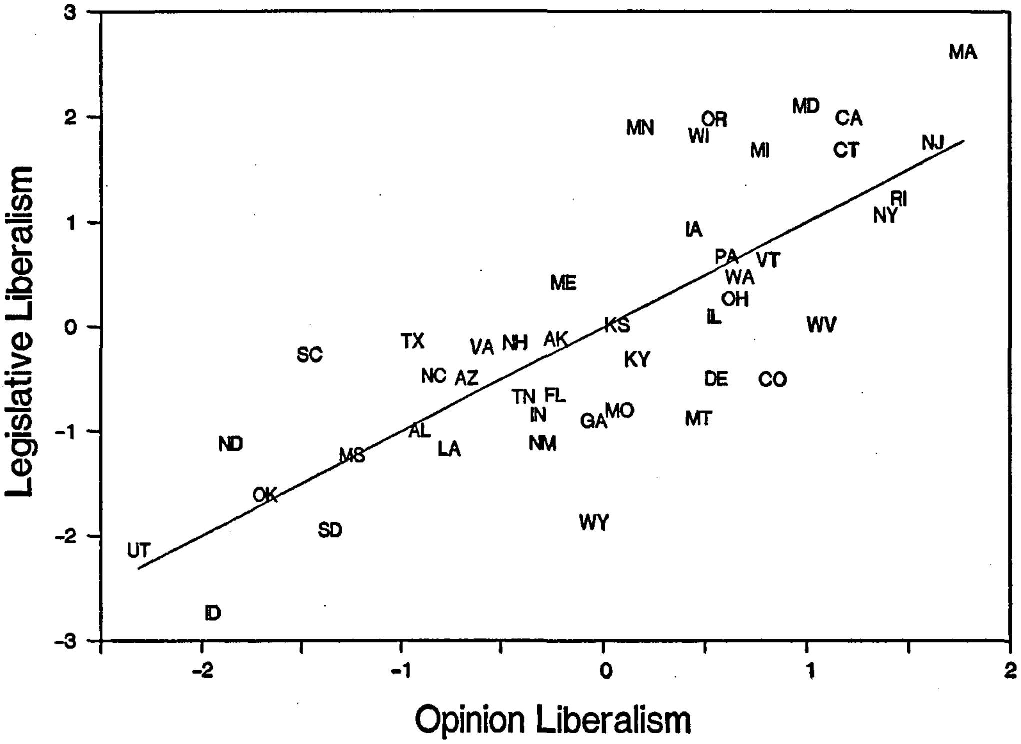
State Opinion, State Parties, and the Legislature

Legislative policy preferences should reflect the policy positions of the two state parties and the two parties' relative legislative strength. Indeed, that is how we measure legislative preferences: as the sum of the state's Republican and Democratic elite positions, each weighted according to the party's average legislative strength. Figure 4 shows these linkages from state party positions and Democratic legislative strength to legislative liberalism, in the form of the large beta coefficients (1.05 and .63, respectively) that can be observed. Of course, these coefficients must be interpreted modestly since legislative preferences are defined in terms of party positions and the legislative party division.

Recall that party positions, summarized as the party midpoint, correlate at .69 with state opinion. Now, with the parties reweighted according to their legislative strength, the measure of legislative preferences correlates at an even higher .79 with state opinion. The working of partisan elections accounts for this increase: the more liberal the parties relative to state opinion, the more the electorate will choose the more conservative Republican party over the more liberal Democratic party.[6] Figure 7 presents the scatterplot of the .79 correlation between state opinion and legislative liberalism.

So far, the data show a pattern that comfortably matches our theoretical expectations. State parties select ideological positions that in part are responses to anticipated electoral sanctions. Then, the state electorates' enactment of these sanc-

Figure 7. Legislative Liberalism by State Opinion



tions result in a mix of Democratic and Republican legislators that enhances further the representation of state public opinion. The final task is to observe the translation of legislative preferences into public policy.

Legislative Elections, Legislative Preferences, and State Policy

We posit three variables to influence state policy directly: legislative liberalism, Democratic legislative strength, and state opinion. The coefficients for these variables can be seen in Figure 4 and Table 2. As Table 2 shows, the three independent variables together account for a large 81% of the variance in policy liberalism. Figure 8 illustrates these effects in terms of scatterplots of the partial regressions and of the multiple correlation between predicted and actual policy liberalism.[7]

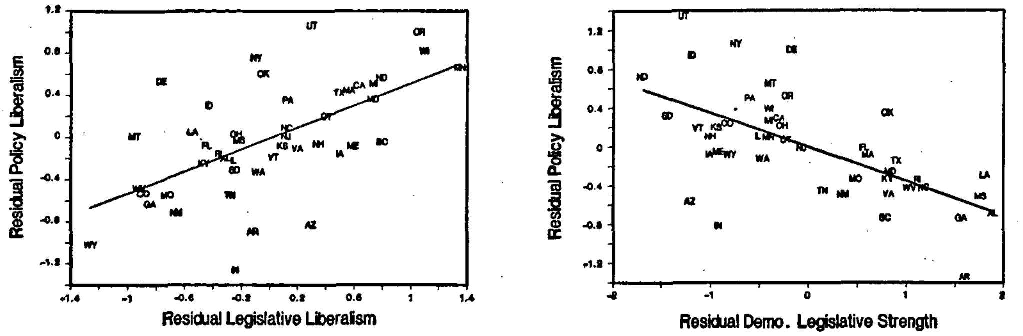
As expected, the liberalism of the state legislature helps determine the direction of state policy but with a beta coefficient of only .52, suggesting that other variables also contribute.[8] One of these variables is state public opinion, which exerts a direct effect (.41) on policy liberalism. This effect presumably represents responsiveness by the state legislature to pressures of public opinion independent of legislative preferences. The most provocative influence is the negative coefficient (−.35) for the effect of Democratic legislative control on policy liberalism.

A negative coefficient for Democratic legislative strength is not entirely unexpected, given that legislative preferences

Figure 8. Accounting for Policy Liberalism

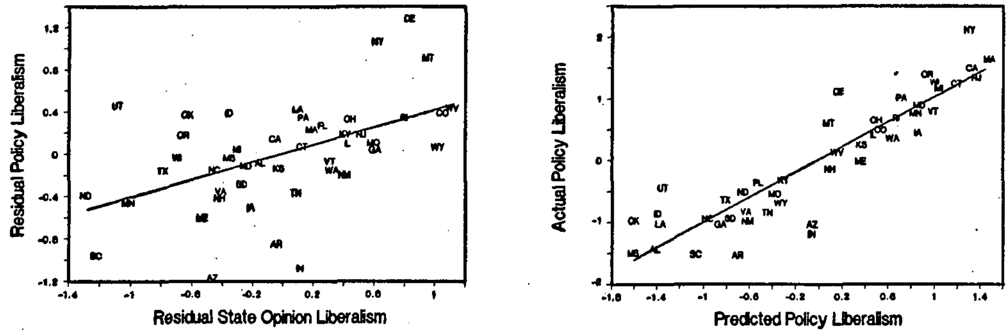
a. Policy by Legislative Liberalism

b. Policy by Democratic Legislative Strength



c. Policy by State Opinion

d. Policy by Predicted Policy



and state opinion are adequately controlled. As we have observed, even with the evident influence of state opinion, state parties tend to stand quite distant from the center of state opinion. Therefore, we might expect each party to moderate further when translating its positions into policy.

From the coefficients in Figure 4, legislative partisanship influences policy in two ways. Obviously, the more Democrats the voters elect, the more liberals they elect. And liberal legislatures lead to liberal policies. We see this from the indirect path of about .32: .63 from Democratic strength to legislative liberalism times .52 from legislative liberalism to policy liberalism. The second way is the direct path (−.35) from Democratic legislative strength to policy liberalism. Once again we have two processes that evidently cancel out. The results suggest that the partisan division of the legislature is of little relevance after all, as if, once in office, Democrats and Republicans start acting so much like each other that one cannot tell the difference![9]

Admittedly, some question must remain concerning the accuracy of the estimated negative impact of Democratic legislative strength on policy liberalism. On the one hand, the coefficient for Democratic legislative strength has the strongest t-value (−5.18) of the three predictors of policy liberalism. Assuming correct specification of the model, the negative coefficient is highly significant. On the other hand, one can suspect possible bias from misspecification of the model.

Southern regionalism is a natural suspect as a confounding influence. To the extent that the South's history of Democratic success but conservative policies is due to the unique southern culture, it is necessary to control for southern regionalism. Indeed, inclusion of a simple dummy variable for the eleven former Confederate states reduces the coefficient from Democratic legislative strength to policy liberalism from −.35 down to −.23. Removing the eleven former Confederate states entirely from the analysis pushes the coefficient down to −.20. (See Appendix B for the complete set of coefficients without the South).

Under these and alternative model specifications, the negative path from Democratic legislative strength to policy liberalism can be pushed toward statistical insignificance.[10] Yet the coefficient does not go to zero nor does its sign change. Thus the net effect of party control on policy remains somewhat ambiguous. The state electorate's choice for state legislature seems to be to elect Democrats who are liberal personally but in office trim to the right or to elect Republicans who are conservative personally but in office trim to the left. The results with the regional control suggest that on balance, Democrats may make more liberal policies but that the statistical evidence must be viewed with caution.[11]

Direct Democracy

So far, we have ignored the direct effect of state opinion on state policy (beta = .41) independent of the electoral paths discussed. Even with the ideological and partisan composition of the state legislature held constant, state opinion exerts an independent influence on state policy. At first glance, this "direct" effect of opinion on policy may seem to reflect sources of representation that are independent of the electoral process. But let us consider how the electoral process may underlie this "direct" effect as well.

Conceivably, when legislatures follow public opinion over their own collective judgment, they do so because legislators believe that they should play the role of instructed delegates. But a more likely reason for following public opinion is the fear of electoral sanctions when they do otherwise. To some extent, the "direct" effect of state opinion may also reflect the unmeasured role of state governors in the representation process: state electorates presumably tend to elect governors with whom they agree with politically and who try to respond to the voice of state opinion. Finally, the "direct" effect of state opinion can reflect direct democracy, in the form of referendum voting.

To a degree that should not be ignored, citizens can participate directly in state policy making. About three hundred referenda and initiatives come before state voters during every legislative biennium. Perhaps the best-known recent example of direct citizen input into state government was the 1978 wave of tax and spending limit votes in 15 states following the passage of Proposition 13 in California. But many other issues came before state electorates in the late 1970s and early 1980s (Magleby 1984; Ranney 1978; for specific issues and outcomes, consult *Congressional Quarterly Weekly Reports*). In various states, voters made policy decisions on gambling laws (from casinos to horseracing to lotteries), bottle bills, utility regulation (with specific emphasis in many states on nuclear power regulation), public smoking, anticrime measures (death penalty, handgun control, bail reduction, and prison construction), teacher homosexuality, "right-to-work" laws, state funding of abortion, voluntary prayer in schools, and state equal rights amendments. In sum, state electorates had the opportunity to alter a number of policies, including many that compose our measure of state policy liberalism.

Conclusion

We have helped to explain why party control of the state legislature is not a particularly good predictor of state policy. The answer is not that electoral politics is unimportant but precisely the opposite. We have seen strong evidence that (1) party positions respond to state opinion, (2) state elections reward and punish state parties based on their responsiveness to public opinion, and (3) Republican and Democratic legislators moderate their positions when making policy. Ironically, although state Republican and Democratic parties tend to represent ideological extremes, they also respond to state opinion—perhaps even to the point of enacting similar policies when in legislative control. This is, of course, exactly what the Downsian model of the electoral process says that electorally responsive parties should do. It is just that this result is achieved by a circuitous process.

We suggest a new way of thinking about state politics. At the state level, the Democratic and Republican parties offer an ideological choice but also respond to state opinion. How well they respond helps to determine their electoral success at the legislative level and also the content of state policy. State politics—in the sense that term is usually understood—does matter.

Appendix A: A Model of State Elections

Our theoretical approach borrows from the well-known electoral theory of Anthony Downs (1957). Downs showed that an issue-motivated electorate would drive political parties toward the center of the political spectrum, thereby providing policy representation no matter which party won elections. In a perfectly Downsian world, elections would in fact be ties. As one can readily observe, the pure form of the Downs model does not apply to state politics in the United States. In each state, the Democratic and Republican parties are ideologically distinct—not the Tweedledum and Tweedledee that the Downs model would predict. Moreover, the U.S. electorate is far less ideologically preoccupied than the Downs model supposes.

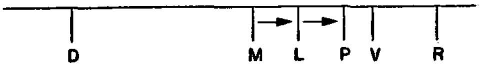
In its pure form, the Downs model assumes that voters are motivated *solely* by ideology and that parties are motivated *solely* with winning elections. Reality is more complicated. Voters are motivated by ideology plus other considerations. In the language of spatial modeling, voters respond probabilistically, not deterministically, to their relative distances to party positions. Parties (strictly speaking, the candidates who run on the party labels) are motivated by the Downsian goal of electoral success, plus other considerations (notably activist opinion) that pull the party toward its ideological extreme.

Here we present a model of state legislative elections that takes into account this mix of Downsian and non-Downsian motives for state electorates and state legislative parties. Figure A-1 illustrates the model as it might apply to a state where the party midpoint happened to be to the left of state opinion.

In this example, a party midpoint (M) to the left of state opinion (V) causes the electorate to elect more Republicans, which pushes the legislative position (L) rightward of M, toward V. This electoral response may be sluggish, resulting in L only some fraction of the distance between M and V. Public opinion would pressure further movement toward V, so that policy (P) is to the right of the legislature (L) in the direction of V.

This model puts all the variables on one unidimensional ideological scale. In practice, one cannot measure the ideological positions of state parties, legislatures, and policies on such a common scale. Here the common scale is a heuristic device, to generate propositions about the relationships

Figure A-1. Hypothetical State: Party Midpoint to Left of State Opinion



Note: V = the electorate's mean (or median) ideological position; D = the Democratic party's public ideological position; R = the Republican party's public ideological position; M = the midpoint between the two parties' ideological positions; L = the legislature's ideological position, resulting from the electorate's partisan vote; and P = state policy.

Figure A-2. With Party Positions Held Constant, Elections Push Legislative Ideology toward State Opinion

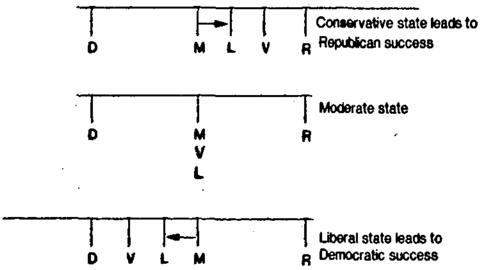

Conservative state leads to Republican success
D
M
L
V
R
Moderate state
D
M
V
L
R
Liberal state leads to Democratic success
D
V
L
M
R

Figure A-3. Parties Respond to State Opinion

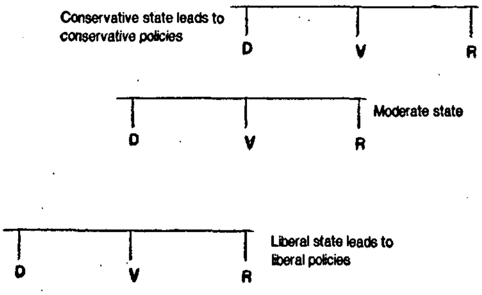

Conservative state leads to conservative policies
D
V
R
Moderate state
D
V
R
Liberal state leads to liberal policies
D
V
R

Figure A-4. With State Opinion Held Constant, Elections Push Legislative Ideology toward State Opinion

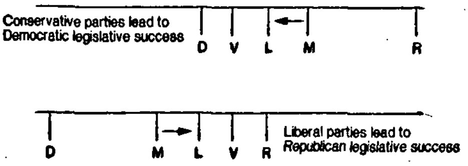
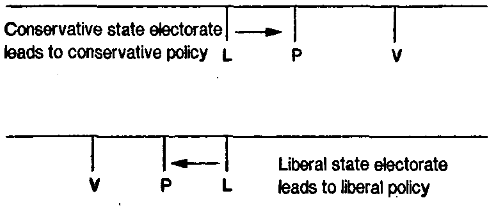

Conservative parties lead to Democratic legislative success
D
V
L
M
R
Liberal parties lead to Republican legislative success
D
M
L
V
R

Figure A-5. With Legislative Ideology Held Constant, Policy Moves in Direction of State Opinion


Conservative state electorate leads to conservative policy
L
P
V
V
P
L
Liberal state electorate leads to liberal policy

among state electoral variables that do not require one uniform ideological scale.

Suppose, for example, that party positions are constant across states, as in Figure A-2: liberal states elect more Democrats, resulting in a legislature to the left of the party midpoint. And conservative states elect more Republicans, resulting in a legislature to the right of the party midpoint.

But state parties can vary their positions in response to state opinion, as illustrated in Figure A-3: liberal states get more liberal parties and conservative states get more conservative parties.

State parties will not always move in perfect symmetry with the electorate. Figure A-4 shows the consequences of different party positions for the same electorate. Relatively liberal parties result in Republican elections and a legislature to the right of the party midpoint. Relatively conservative parties result in Democratic elections and a legislature to the left of the party midpoint.

As in Figure A-4, the ideological position of the legislature (L) will not necessarily match that of the state electorate. To the extent that the legislature is out of alignment with state opinion, the legislature should respond in the direction of state opinion when making policy. Put another way, with the degree of legislative liberalism held constant, state opinion should influence the policy direction, as in Figure A-5.

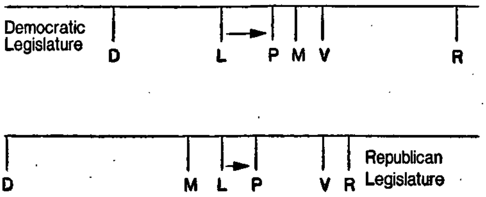
The partisanship of the legislature should make a difference, too. With legislative ideology and state opinion held constant, the more Democratic the legislature, the more electorally motivated it would be to shift its policy direction to the right rather than the left. Consider Figure A-6, which shows two states with the same state opinion (V) and the same legislative ideology (L). In each, the legislature and the party midpoint (M) are (arbitrarily) to the left of state opinion. The legislature in the first example is Democratic (L to the left of M) while that of the second is Republican (L to the right of M).

Since both legislatures are to the left of state opinion, each should move to the right in its policies. But legislative Democrats would be far to the left of state opinion and therefore be motivated to move to the right, while legislative Republicans would be somewhat to the right of state opinion and somewhat motivated to move to the left. Summing these vectors, the Democratic legislature would choose policies farther right (in the direction of state opinion) than the Republican legislature under similar circumstances.

We emphasize again that we cannot observe the placement of parties, legislatures, policies, and the electorate on a common ideological scale as depicted above. We do not even know, for instance, whether state legislatures generally are to the right or to the left of state opinion. But Figures A-2–6 provide the basis for predictions of covariations.

To summarize, Figure A-2 illustrates how ideologically fixed parties should result in liberal electorates' electing more Democrats. Figure A-3 illustrates how when state parties can be ideologically flexible, their ideologies should correlate with state opinion. Figure A-4 suggests that with state opinion held constant, liberal parties result in the election of more conservative Republicans and conservative parties result in the election of more liberal Democrats. Figure A-5 illustrates how state opinion should influence policy direction even with legislative positions held constant. And Figure A-6 shows how with legislative positions and electoral preferences held constant, Democratic legislatures should produce more conservative policies than Republican legislatures.

Figure A-6. With State Opinion and Legislative Positions Held Constant, More Republican Legislature Is More Liberal



Appendix B: Taking Out the South

Deleting southern states unambiguously removes any complication from the effects of southern culture. Table B-1 shows the correlation matrix with southern states excluded. Note how better behaved this matrix is, given our usual expectations about electoral politics. Both state party identification and Democratic legislative strength now are correlated positively with state opinion and state policy. The overall correlation between opinion and policy remains strong at .79.

The non-South standardized regression equations are shown in Table B-2. The path model is shown graphically in Figure B-1. In general, the coefficients for 35 nonsouthern states are similar to those for all 46 examined states but with a few exceptions. The effect of state party positions on party identification and on legislative partisanship are considerably smaller. These coefficients reflect that Democratic and Republican party leader-ships do not show as much ideological variation when the South is excluded.

Most important, however, is that with southern states excluded, the direct effect of legislative partisanship on policy is modest (−.20). As before, this suggests that Democratic and Republican legislators moderate their positions when in office. However the −.20 coefficient does not override the indirect effect of legislative partisanship on policy via the preferences of the legislature (.49 x .64 = .31). Thus, we have further evidence that the election of Democrats to the state legislatures may lead to more liberal legislation after all.

We display the relationships for the non-South in standardized format because this format provides the best sense of how the variables relate to one another with the South removed. Left undone is a formal accounting of the possible additive and interactive effects of the South. When a South dummy is added to

Table B-1. Correlations without the South

Variables	Opinion Liberalism	Party Elite Liberalism	Democratic Party Identification	Democratic Legislative Strength	Legislative Liberalism	Policy Liberalism
Opinion liberalism		.65	.39	.55	.80	.79
Party elite liberalism			-.07	.10	.86	.78
Democratic party identification				.79	.32	.12
Democratic legislative strength					.57	.38
Legislative liberalism						.83
Policy liberalism						

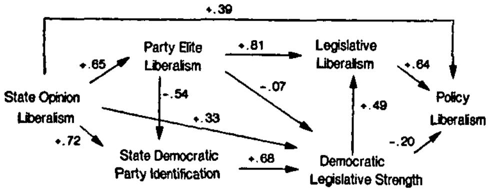
Table B-2. Standardized Regressions without the South

	Dependent Variable				
Independent Variables	Democratic Elite Liberalism	Democratic Party Identification	Democratic Legislative Strength	Legislative Liberalism	Policy Liberalism
Opinion liberalism	.64*** (.13)	.72*** (.18)	.33* (.16)		.39* (.15)
Democratic elite liberalism		-.54** (.18)	-.07 (.15)	.81*** (.02)	
Democratic party identification			.68*** (.13)		
Democratic legislative strength				.49*** (.03)	-.20 (.11)
Legislative liberalism					.64*** (.15)
Adjusted R^2	.40	.30	.67	.98	.74

Note: Standard errors are in parentheses.

*p < .05, two-tailed test.
**p < .01, two-tailed test.
***p < .001, two-tailed test.

Figure B-1. Path Model of the State Policy Process (Non-South)



the original equations, it shows a significant additive (pro-Democratic) effect on state party identification and on Democratic legislative strength, plus a significant negative effect on party elite liberalism. (The suggestive, negative relationship with state policy liberalism is not significant). When South interaction terms (e.g., South × state opinion) are added, they suggest a more sluggish representation process in the South but do not pass the Chow test for statistical significance.

Notes

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1. Four states have been eliminated from the analysis. Three of these—Alaska, Hawaii, and Nevada—were eliminated due to limitations in our measurement of public opinion (Wright, Erikson, and McIver, 1985). Nebraska is excluded because of its nonpartisan legislature.

2. This argument may apply to cross-national comparisons as well. The U.S. Democratic party is more ideologically centrist than European parties of the Left, and more successful electorally. One may wonder, therefore, whether the relative electoral success of the left, but nonsocialist, U.S. Democrats (and also the Canadian Liberals) disturbs the cross-national relationship between leftist party success and policy. However, cross-national studies do not code the nonsocialist U.S. Democrats and Canadian Liberals as parties of the Left. The Leftist designation is reserved for explicitly socialist or social democratic parties. If cross-national studies were to code the U.S. Democrats and Canadian Liberals as successful leftist parties, cross-national relationships between party control and policy would become seriously attenuated.

3. We speak somewhat loosely about movement to the political center. Strictly speaking, attempting to observe whether parties are at the political center would require a common metric for parties and the electorate. In actuality, the location of the center is irrelevant, since each party should be electorally rewarded for moving toward the ideological position of its opposition, whatever side of center. See Appendix A.

4. The party midpoints for the four indicators of party elite liberalism correlate among themselves in a range from .71 (state legislators and county chairs) to .83 (congressional candidates and local chairs). The midpoints correlate with the composite tally in a range from .87 (county chairs) to .93 (state legislators). Within party, the correlations range from .52 to .73. A principal components analysis of the eight state-level indicators of elite ideology yielded two distinct factors. As expected, these components identify the ideological constraint within parties as well as philosophical distinctiveness of the Democrats and the Republicans. The rotated principal components are as follows:

Party elites	F1	F2
Republican		
State legislators	.87	.19
National delegates	.64	.35
Congressional candidates	.84	.20
State and local party officials	.81	.27
Democratic		
State legislators	.35	.77
National delegates	.32	.72
Congressional candidates	.56	.72
State and local party officials	.06	.91

5. Certain arrows (e.g., from state party identification to legislative liberalism) are deleted from Figure 4, on the grounds that the expected direct effect is zero. Technically speaking, therefore, Figure 4 presents an *overidentified* model. Inclusion of the omitted effects would be of little consequence other than to clutter the analysis with small, theoretically meaningless coefficients. One possible exception, however, is a persistent negative relationship between state party identification and policy liberalism when other variables are controlled (see n. 10).

Still another alternative specification would be to separate the indicator of party elite liberalism into separate variables. When all four indicators (based on legislators, congressional candidates, convention delegates, and county chairs) are used as separate independent variables, in each case the adjusted R-squared goes down, a clear diagnostic sign that the decomposition is ill advised. Division into two separate variables representing candidates (congressional candidates and state legislators) and party leaders (delegates and chairmen) has some appeal. However, the correlation of .82 between these separate two-indicator composites results in too much multicolinearity to disentangle any separate effects.

6. Testing this proposition directly requires the measurement of state party positions and state opinion on a common scale. Suppose we assume the metric that we used for party positions for the construction of Figure 5. Using this scale, we should find the least Democratic success when the midpoint between the parties is more liberal than state opinion. Indeed, the correlation between the party midpoint-state opinion difference and Democratic legislative strength is strongly negative (−.63). In 38 of the 46 states, whether the midpoint between the parties is to the left or to the right of state opinion predicts whether the Democrats win more or less than their average percentage of the legislative seats.

7. For the illustrations of the partial regressions in Figure 8, each independent variable is depicted as the deviation from the value predicted from the other independent (control) variables. The dependent variable (policy liberalism) is also residualized as a deviation from the value predicted by the control variables.

8. For an alternative demonstration that bypasses the measure of legislative liberalism, consider the regression of state policy liberalism on party elite liberalism, state opinion, and Democratic legislative strength. From this equation, the coefficient for Democratic legislative strength is a negligible −.02. Thus, holding constant both state opinion and the positions of the state parties, Democratic legislative strength does not predict more liberal policies.

9. Just as we have a measure of legislative liberalism, based on weighting state party positions by their legislative strength, so, too we can measure the ideological positions of *losing* legislative candidates, weighting party positions by the legislative strength of their opponents. Gratifyingly, when this variable is added to the policy equation, it yields a coefficient of only .01. The ideological positions of state parties matter only in proportion to their electoral strengths.

10. Apart from controlling for region, the one other specification found to reduce the negative coefficient for legislative liberalism appreciably was to control for state party identification. Democratic party identification is somewhat negatively related to policy liberalism, even outside the South. This oddity suggests the possible presence of some unmeasured variable that accounts for both Democratic strength and policy conservatism, causing these two variables to covary.

11. The effect of party control on policy conceivably varies across states, with party control being most important in states where parties approximate the ideal of programmatic, disciplined parties. Perhaps in such states we would find that Democratic legislative success has a direct positive effect on policy liberalism. Accordingly, we reexamined the party effect after sorting on several measures of state party characteristics. We sorted on party competition (Ranney 1971), legislative party cohesion (LeBlanc 1969), state party strength (Cotter et al. 1984), party diversity (Garand 1985), interparty policy differences (using our own measures), and polarization of party identification based on ideology (from our state opinion data). When examining the top scoring states on each of these variables, we still found in each instance the estimated direct impact of Democratic legislative strength on policy liberalism to be negative.

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REPUBLICAN SCHOOLMASTER: THE U.S. SUPREME COURT, PUBLIC OPINION, AND ABORTION

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The United States Supreme Court has a historical role as a "republican schoolmaster," inculcating virtues in the citizenry. The role as teacher to the republic also serves the interests of the Court. As the "weakest branch," the Supreme Court needs public support if its decisions are to be effective. We investigate the Court's ability to win popular support for its rulings, specifically in the case of Roe *v.* Wade. *The analysis shows that the Court's decision did affect public attitudes but not as previous work would predict. While support for abortions to protect health increased as a result of the Court's decision, the public became more polarized over "discretionary" abortions. The puzzle is what process can account for these disparate reactions. We develop a theory resting on interpersonal influences to explain these results, arguing that the social interpretation of events drives the differing outcomes. This theory is then tested against a purely psychological alternative. The closing discussion considers how these results can be extended to the general problem of public decisions and popular responses, including presidential actions and the influence of the media.*

There is a long tradition of research dealing with the relationship between public opinion and the courts. Much of this research has focused on the impact of public opinion on the behavior of judges. This is a natural result of the problem of the role of the courts in a democratic society. Responsiveness of courts to the views of the public is based on both normative and pragmatic considerations. The courts *should* be responsive in a democratic society. The courts *must* be responsive because of their weakness as institutions. The tools available to the courts to enforce their will on a resistant public are few (Bickel 1962). But if, through their opinions, the courts can instruct the public, enforcement is gentle but effective. We consider this relationship of court and public—the role of the United States Supreme Court as "republican schoolmaster."

Nowhere has the question of the relation between public opinion and the courts been discussed more than in regard to the role of the Supreme Court. The Anti-Federalists recognized and distrusted the power of the Court as ultimate interpreter of the Constitution, but in *Federalist* no. 78 Hamilton argued that the judiciary lacked the power to enforce its decrees and that it was therefore the "weakest branch." Moreover, he argued, the Court would provide a valuable protection against the evils of majority tyranny.

Tocqueville, in his observations on American life, supported Hamilton's argument (Tocqueville 1969, 268–69). Tocqueville saw the judiciary as an aristocracy of sorts and thus a valuable

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source of the aristocratic virtues that threatened to disappear completely in democratic society. It is clear that the Fed- that threatened to disappear completely in democratic society. It is clear that the Federalists and Tocqueville envisioned a role for the Supreme Court beyond that of servant to the public will. Indeed, the concern at the founding was not only that the Court should respond to public opinion but that it should also play an important role in educating that opinion.

The conception of the Court as republican schoolmaster generally reflects the notion that the Court, through its explication of the law and its high moral standing, may give the populace an example of the way good republicans should behave. Indeed, Lerner (1967) argues that this role was consciously recognized by the justices themselves during the early history of the Court. This concept has a specific application in the belief that the Court can through its decisions confer legitimacy on the claims of disadvantaged groups.

The question of the Supreme Court and public opinion has received much attention in the literature. However, most studies dealing with public opinion and the Court deal with questions of regime support or the response of the Court to public opinion (Dahl 1957; Kessel 1966; Murphy and Tanenhaus 1969). The studies that do deal with the question of the response of the public to Supreme Court decisions generally focus on elite groups (Birkby 1966; Dolbeare and Hammond 1970; R. Johnson 1967; Kemp, Carp, and Brady 1978). While these are important, our concern is with the broader public. If elites attend to public preferences and the Court fails to convince the public, elite support is likely to be tenuous and short-lived.

Public response to Court decisions can have a significant effect on the willingness of officials to comply with the decisions. Should they choose to do so, public officials might move quickly and decisively to implement the policies defined by Court decisions. However, officials' perceptions that an issue is too controversial can lead to inaction, as was the case with school prayer (Dolbeare and Hammond 1970). It may also result in outright resistance to the Court's mandate, as was the case with desegregation (Rodgers and Bullock 1972). Like the Court, public officials "follow th' election returns," but they have far greater incentives than the Court to abide by that mandate.

The question we address in this paper is the nature of public response to Court actions. The argument comes in three parts. First, we present evidence that the Court does influence public opinion but not as expected by most previous work. This section is unabashedly atheoretical. We then develop a post hoc theory of public response to government actions in general and the Court's in particular. This theory accounts for the perplexing findings of the first section. Finally we offer a test of the theory.

Do Court Rulings Affect Public Opinion?

A prominent explanation of the effect of Court rulings on public opinion is the legitimation hypothesis (Adamany 1973; Casey 1974; Dahl 1957; Kessel 1966; Marshall 1987; Murphy and Tanenhaus 1969). According to this line of argument, when the Court rules, it confers legitimacy on the position it favors. The Court's legitimacy allows it to bestow legitimacy on others through its rulings. This leads to increased public support for the position taken by the Court.

There are attractive features to this theory. The Court is held in high regard by many citizens. It is also recognized as the final arbiter of legal questions. Further, it would be perverse to predict the opposite, that the Court's rulings would foster *disagreement* with its position.

Legitimation is one specific mechanism by which the Court may obtain greater support for its position, but there are many other possible mechanisms. Regardless of the mechanism governing individual reactions, the common hypothesis is that rulings increase support for the Court's position. We want to consider these theories as a class, which we call the positive response hypothesis. For our purposes it is not necessary to specify the precise mechanisms that foster support, only to bear in mind that we are considering a broad category of theories, not the legitimation hypothesis alone.

Studies of the positive response hypothesis have focused on changes in the aggregate level of public support for the Court's position. Blake (1977), Uslaner and Weber (1980), and C. Johnson and Canon (1984) all adopt this approach. They compare the level of public support prior to a decision with the level of support after the decision. Marshall (1987) considers 18 Court decisions since the 1930s. In his analysis the shift between predecision and postdecision opinion is taken as a dependent variable to be explained by several independent variables such as unanimity of the Court, liberalism of the decision, and the time lag between the polls. While more complicated than others, Marshall's approach still focuses on the simple shift between pre- and postdecision opinion. The problem with this approach, as Uslaner and Weber point out, is that trends could lead to increased support apart from any direct impact of the Court.

Empirical studies of the Court's impact have produced little support for the positive response hypothesis. Murphy and Tanenhaus (1969) point to the low salience of the Court as a barrier to influence. Adamany (1973) cites a long list of disappointed attempts to demonstrate the existence of such influence. More recently, Marshall's (1987) study of 18 court decisions found that the average shift following the decision was virtually zero. However, Marshall did find some positive evidence in the case of liberal, activist decisions, after a time lag. C. Johnson and Canon (1984, 12–13) show that support for legal abortions increased slightly after the Court's *Roe* decision. The mixed evidence raises doubts about the Court's impact.

Our position is that increased aggregate support for the Court's position is not the best indicator of impact. In the wake of a decision some people may respond positively and some negatively. Both groups are affected by the Court's ruling even if the net effect is no change in aggregate opinion.

Our alternative to the positive response hypothesis is to focus on the *structure* of opinion concerning a ruling; that is, we are concerned with which groups support and oppose a position and how intensely. If support rises (or falls), the decision had an effect. But even if the net effect is nil, while the structure of group support and opposition changes, we argue that the Court still had an impact. We call this the structural response hypothesis.[1] The null hypothesis, then, is that in the wake of a decision the structure of opinion is unchanged. This truly would be evidence that the Court is a poor schoolmaster—not that it fails to convert its pupils but that it leaves them unchanged.

Our position is that theories emphasizing, for whatever reasons, increased agreement with the Court's rulings miss the political role of the Court. When the Court rules on politically controversial cases, it may establish the law of the land, but it does not put an end to debate. It neither converts the opposition nor ends the controversy. A satisfactory theory of Supreme Court impact must recognize that Court decisions do not necessarily bring about agreement and may instead sow the seeds of dissension.

Testing the Structural Response Hypothesis

One problem with the study of public response to the Court is that only a small portion of the public is aware of most Court decisions, and an even smaller proportion is accurately informed about them (Kessel 1966; Murphy and Tanenhaus 1969). Therefore, an examination of Court impact on public opinion is meaningful only in cases where public attention is high. For example, Caldeira (1987) shows that during the Roosevelt Court-packing period the public did respond to Court decisions by adjusting their approval of the Court. This is evidence that when the issues are salient, the public may respond to Court actions. We would expect to see a Court impact in relation to issues of great controversy and public interest, such as desegregation and school prayer, rather than issues of low interest such as antitrust or the commerce clause. Paradoxically, however, the high interest that makes a Court decision salient may also blunt its impact, since these issues are likely to tap relatively well-ordered belief structures.

To test the structural response hypothesis, then, we need a visible issue engaging seemingly well-informed public attitudes. A more visible, controversial, salient decision than the Court's 1973 abortion decision, *Roe* v. *Wade,* could hardly be found.

Roe is easily the most highly debated and criticized decision the Court has made in the last 20 years. Annual commemorative marches are held by those opposed to it, constitutional amendments to overturn it have been proposed, and the author of the Court's opinion—Justice Harry Blackmun—has been the target of hate mail and death threats. Thus, the decision is ideal for a study of public opinion impact. Because of the controversy surrounding it, the level of awareness about the decision should be high.

Of course, this also means that we are observing a high point of Court impact on public opinion. But the emergence of the pro-choice and pro-life movement in the wake of the decision and much of the activism surrounding it occurred after the collection of our data. Our work is concerned with the immediate aftermath of the ruling, not the long-term developments.

Studies of public opinion concerning abortion have examined levels of support in the population but have often ignored the issue of Supreme Court impact (Granberg and Granberg 1980; Plutzer 1986). Where attention has been paid to the decision's impact on public opinion, studies have adopted the positive response hypothesis and found little support for it (Blake 1977; Combs and Welch 1982; Uslaner and Weber 1980). As we have argued above, this conceptualization of public opinion impact is too narrow. In examining the Court's impact on public opinion, we need to consider the structural response hypothesis as well.

The Problem of Attribution

Any study of public response to Court decisions faces a difficult design problem. If we compare opinion before the ruling with opinion after, how do we know that changes we find are due to the Court's ruling rather than something else? For example, Uslaner and Weber (1980) agree that there was more support for some types of abortion after the *Roe* decision than before. But they discount this finding on the grounds that support was trending up before the decision, thus offering a plausible alternative interpretation of the data. How do we sort out the impact of the decision from the spurious influence of other factors? To what do we attribute the change?

This problem is not unique to studies of the Court but applies equally to other public decisions and events. The methodological basis of the problem is that we have a quasi-experimental design, with premeasure, experimental treatment, and postmeasure. The problem is that public decisions typically affect all citizens, so there is no contrast group not exposed to the treatment. Thus, any change between measurements might be due to the treatment but might equally be due to something else. It is very difficult to rule out the possibility of spurious influences.

We gained leverage on the problem by distinguishing between those who had heard of the *Roe* decision and those who had not. In order to respond to the Court's action, one must have heard of it. Those who have not heard of it are in effect our contrast group. If long-term trend accounts for change in opinion, those who had not heard of the decision should resemble those who had. If there are shifts among those who heard while those who did not remain unaffected, there is reason to attribute the change to hearing of the Court's decision.

This is not a perfect solution, because respondents are not randomly assigned to the contrast group. Thus, the differences we find could be due to differences in composition between the experimental and contrast groups. For example, those who do not hear of Court decisions are undoubtedly less educated on average than those who do. Since education is also correlated with opinion on the abortion issue, lower support for abortion among those who do not hear may be due to their lower educational level rather than lack of exposure to the decision. However, if we model abortion opinion as a function of several variables, including education, the differences we find are much less likely to be due to composition, precisely because we explicitly include the compositional variables in our model.

Our design is a workable, if not perfect, solution to the attribution problem. By introducing a comparison group we circumvent the problem of such spurious influences as trend. And by explicitly including composition variables we compensate for nonrandom assignment to the comparison group.[2]

Model Specification

The structural response hypothesis we consider concerns the impact of the Court's *Roe* decision on the structure of public support for abortion. As discussed above, the Court's influence may be found in changes in the pattern of support or opposition, as well as in the aggregate level of support that the positive response hypothesis considers. The model we estimate allows both of these potential effects to be specified and tested.

In order to test the structural response hypothesis we need to relate individuals' group membership and personal characteristics to their attitudes toward abortion. The model we estimate uses observations from two cross-section samples, one taken prior to the Court's ruling and the other taken shortly after. The data from these two surveys are pooled and the equation estimated is

$$\begin{aligned} y = \alpha_1 + \alpha_2 d_1 + (\beta_{11} + \beta_{21} d_1) Z_1 \\ + (\beta_{12} + \beta_{22} d_1) Z_2 + \ldots \\ + (\beta_{1k} + \beta_{2k} d_1) Z_k + u, \end{aligned}$$

where y is the respondent's position on the abortion scale, d_1 is a dummy variable equal to one after the Court's decision and zero before, the Z_k variables are personal characteristics of the respondent that influence preferences on the abortion issue, and u is a random disturbance term.

The coefficients in this model are specified so that they may vary between the predecision and postdecision samples.

The β_{1k} coefficients represent the effect of Z_k before the decision, that is, when d_1 is equal to zero. The β_{2k} coefficients indicate the additional influence of Z_k following the decision, when d_1 equals one. Thus, the effect of, say, Z_1 before the decision is β_{11} and its influence after the decision is $\beta_{11} + \beta_{21}$. Clearly if Z_1 has the same influence after the decision as before, β_{21} must equal zero. Conversely, if the effect of Z_1 changes as a result of the decision, β_{21} must be different from zero. This logic is the basis for our hypothesis tests below.

In the equation, α_2 is the coefficient on the dummy variable for 1973 and the β_{2k} coefficients indicate how much change there is in the influence of each Z_k variable between 1972 and 1973. If the structure of preferences was unaffected by the Court's actions, we would expect all the β_{2k} coefficients to be near zero and the effects of Z_k (estimated by β_{1k}) to be constant over the two cross-sections. If each of the β_{2k} coefficients is near zero while α_2 is large and positive, the Court's decision had a uniform effect of raising support for abortion among all population groups represented by Z_k—as predicted by the positive response hypothesis—without changing the structure of the support. If, in contrast, α_2 is small while at least some of the β_{2k} coefficients are large, the primary effect of the decision was to shift the structure of group cleavage on the issue, though producing little net effect on the aggregate balance of preferences. Finally, if both α_2 and the β_{2k} are close to zero, the Court's action had no discernible influence on the public's preferences regarding the issue.

Data

In order to estimate the model we need data taken prior to the Court's decision and again shortly afterward. The National Opinion Research Center's General Social Survey (GSS) provides precisely this. The GSS field work was conducted in February through April of both 1972 and 1973. The Court's ruling was handed down on 21 January 1973. Thus the second cross-section was collected within a short time of the decision, while the first set of respondents were interviewed well before.

The GSS also provides an excellent battery of preference measures for the abortion issue. Each survey contains six relevant items inquiring about approval of legal abortion in cases of (1) threat to the health of the mother, (2) a likely defect in the baby, (3) rape, (4) poor families who cannot afford another child, (5) unwed mothers, and (6) the parents' not wanting more children.[3] In each case the respondent's are asked if they would permit legal abortions in the specified circumstances. We group these into two sets, one including the health, rape, and defect items and the other the poverty, unwed mother, and unwanted cases. We form two simple scales by counting the number of circumstances in which a respondent would permit abortion, one scale for the first three items (the "health" scale) and one for the second three (the "discretionary" scale). Each of these range from zero to three. The distribution of responses on these scales is given in Table 1.[4] These two scales are our dependent variables.

There are several reasons for treating the health and discretionary scales sepa-

Table 1. Distribution of Abortion Scale Responses (%)

	Discretionary Scale		Health Scale	
Score	1972	1973	1972	1973
0	42.1	38.8	10.3	5.5
1	10.9	9.7	7.2	5.3
2	10.2	8.3	10.7	11.9
3	36.8	43.2	71.8	77.4
Number of cases	1,308	1,332	1,308	1,332

rately. The Court's decision broke major new ground on the discretionary side, while legitimizing the more common justifications based on health. It is possible that the decision may have affected preferences concerning the abortion circumstances differently. Further, the distribution of opinion is markedly different on each scale; the health scale reflects near consensus, while the discretionary scale shows sharp division. This becomes an important point in our theoretical development below. Finally, there is some debate concerning the unidimensionality of the six items and a suggestion that they should be kept distinct (see Clogg and Sawyer 1981).

There are a number of reports of previous research concerning the correlates of abortion preferences, many based on these same data (e.g., Granberg and Granberg 1980; Plutzer 1986). We draw on this research in selecting our exogenous variables. Our concern is to capture the major identified influences in order to test the primary hypotheses concerning the influence of the Court's decision. The variables we have included are education, gender, race, religion, and frequency of church attendance.[5]

Estimation

The ordinal nature of the dependent variables makes an alternative to regression desirable. An appropriate technique is the polychotomous probit model discussed by McKelvey and Zavoina (1975). This technique assumes that the responses arise from an underlying continuous variable representing the respondents' preferences regarding abortion. The actual questions asked in the survey cut this underlying dimension at various points, giving rise to the observed responses. The polychotomous probit model estimates the coefficients relating the exogenous variables to the underlying continuous preference dimension (up to a linear transformation). Thus the estimated probit equation may be thought of as exactly like a regression equation except that the scale of the dependent variable (and hence the absolute size of the coefficients) is arbitrary.

The specification we have proposed demands a test of the hypothesis that the coefficients of the model are invariant across the two samples. Its central test concerns the set of β_{2k} coefficients in our equation. The null hypothesis is that the structure does not change after the ruling. This implies that all the β_{2k} coefficients are jointly equal to zero. The test is readily carried out by comparing the probit model in which the coefficients are unconstrained with a model in which the β_{2k} coefficients are constrained to be zero. The change in the likelihood function provides the test: −2 times the change in the log of the likelihood function is distributed as a chi-squared with degrees of freedom equal to the number of constraints imposed (McKelvey and Zavoina 1975, 110–11). This is analogous to the similar test using constrained and unconstrained regression models (see Johnston 1984, 187–90). If this test leads to the rejection of the null hypothesis, we will have evidence for the structural response hypothesis in the wake of the Court's decision.

Empirical Results

We begin by estimating our model first for the 1973 respondents who had heard of the Court's action, then for those who had not. As discussed above, we need to compare those who heard with the contrast group who had not heard. After comparing the results for each group, we turn to a further check on the effects of differing composition in the two groups.

The estimates on which our hypothesis tests are based are presented in Table 2. Here the equations for abortion preferences are presented for both the constrained and unconstrained models and for both abortion scales. This table contains all the 1972 respondents but among the 1973 respondents only those who said they had heard about the Court's decision.

The estimated coefficients agree with the typical findings of previous work, indicating that those with more education are considerably more supportive of abortion than those with less. Nonwhites are substantially more opposed to abortion than whites, and Catholics more than non-Catholics. Further, as church attendance increases, so does opposition to abortion.[6] The coefficient for women is small, indicating that the responses of men and women do not differ substantially. These results hold true for both abortion scales.

Overall, the equations are highly significant, with chi-squared values of 533.9 and 468.4, with 10 degrees of freedom, for the discretionary and health scales respectively. The chi-squared values are calculated using the standard likelihood ratio test. (For the application to probit see

Table 2. Estimated Abortion Scale Equations for Respondents Who Have Heard of the *Roe* Decision (Probit Estimates)

	Discretionary Scale Models		Health Scale Models	
Variables	Unconstrained	Constrained	Unconstrained	Constrained
Intercept	−.49	−.38	.78	.94
	(.14)	(.11)	(.15)	(.12)
1973 dummy	.31	.08	.58	.19
	(.21)	(.05)	(.23)	(.06)
Education	.11	.11	.12	.11
	(.01)	(.01)	(.01)	(.01)
Education*1973	−.00	—	−.02	—
	(.02)		(.02)	
Female	.09	.00	.06	.04
	(.07)	(.05)	(.08)	(.06)
Female*1973	−.18	—	−.05	—
	(.10)		(.11)	
Nonwhite	−.12	−.23	−.46	−.49
	(.09)	(.07)	(.10)	(.08)
Nonwhite*1973	−.26	—	−.06	—
	(.14)		(.15)	
Catholic	−.20	−.32	−.21	−.34
	(.08)	(.06)	(.09)	(.06)
Catholic*1973	−.25	—	−.27	—
	(.11)		(.13)	
Church attendance	−.14	−.14	−.13	−.13
	(.01)	(.01)	(.01)	(.01)
Threshold 1	.29	.29	.41	.41
Threshold 2	.56	.56	.90	.89
log(L)	−2,651.5	−2,656.6	−1,811.1	−1,814.6
Chi-squared	—	10.2	—	7.0
Significance	—	$p < .05$	—	ns

Note: Standard errors are in parentheses. Number of cases = 2472.

McKelvey and Zavoina 1975, 110–11).

The test of the structural response hypothesis, comparing the constrained and unconstrained models, is presented at the bottom of columns 2 and 4 in Table 2. The first test considers the discretionary preference scale. Here the chi-squared value of 10.2 with 4 degrees of freedom is statistically significant, indicating that we should reject the null hypothesis of no structural change. The coefficients in column 1 indicate that nonwhites and Catholics became substantially more opposed to discretionary abortion in the aftermath of *Roe.* While the coefficients are not large, there is also a suggestion that women, who had been slightly more supportive of abortion than men, may have become slightly more opposed. There is scant evidence that the effect of education changed.

While it is clear that the structure of preferences for discretionary abortions changed in the direction of greater group differences and conflict, it appears that there was little or no increase in support across all groups. The coefficient for the 1973 dummy variable is positive, but not much larger than its standard error. This suggests that in the case of discretionary abortions, the Court's impact was primarily to increase the polarization of groups on the issue, with little or no increase in overall approval. While consistent with the structural response hypothesis, this result is inexplicable under the positive response notion.

The results are different in the case of the health scale. Here the chi-squared value in column 4 is not significant, indicating that we cannot reject the null hypothesis of constant coefficients. Thus, the polarization effect seen in the case of discretionary abortions is not present in this case. However, there is evidence of an across-the-board increase in approval of health-related abortions. The coefficient for the 1973 dummy is quite large relative to its standard error. This implies that while group differences were not substantially affected, there was an overall increase in approval for abortions in cases of threat to the mother's health, rape, and probable birth defects. This is, of course, what the positive response hypothesis would predict.

As discussed above, a potential design problem is the assumption that the actions of the Court are the only explanation for the change in public attitude, for other trends present before the decision may well have produced the changed coefficients, quite independently of the Court's action. Others have discounted *Roe's* effects on public opinion on the grounds that the trend in the years preceding *Roe* was increasingly favorable to abortion (Blake 1977; Uslaner and Weber 1980; but cf. C. Johnson and Canon 1984). Clearly, our results, too, might be colored by such a trend.

We can test this possibility. There were 168 respondents in 1973 who said they had heard nothing of the Court's ruling. If the structural shifts we find are due to changes independent of the Court's action, these comparison group respondents should demonstrate the same structural shifts as the respondents in Table 2 who had heard of the decision. If they do not, it is additional evidence that the shifts found in Table 2 are in fact the result of public response to the decision.

The results for those who had not heard of the Court's decision are presented in Table 3. There is no evidence for structural shifts like those found in Table 2. In the case of the discretionary scale, there is virtually no change evident at all; and for the health scale, too, the chi-squared falls considerably short of significance. In neither case does the coefficient for the dummy variable for 1973 approach statistical significance. Hence, among those who did not hear of the *Roe* decision, neither the structure of support nor the level of support for abortion changed from 1972 to 1973. This is additional evidence that the shifts found in Table 2 are indeed the result of public reaction to the decision, not the spurious result of independent forces.

But perhaps the Court's influence was felt mainly among the more interested and educated. Not surprisingly, those who had not heard of the Court's decision are less educated than those who had heard and undoubtedly less politically involved. Among those who had heard of the decision, the mean education was 12.0 years of school versus a mean of 10.2 years among those who had not heard. Our finding of little change among those who had not heard of the ruling might be due to their lower interest or education rather than simply to their not hearing of it.[7] This possibility is mitigated by the fact that we include education as a predictor variable. However, it is conceivable that inclusion of the education variable does not capture all education effects. In order to check for this possibility, we can stratify the 1973 sample by education.

We can test the education objection by

Table 3. Estimated Abortion Scale Equations for Respondents Who Have Not Heard of the *Roe* Decision (Probit Estimates)

	Discretionary Scale Models		Health Scale Models	
Variables	Unconstrained	Constrained	Unconstrained	Constrained
Intercept	-.47	-.49	.69	.67
	(.14)	(.13)	(.15)	(.14)
1973 dummy	-.20	-.04	.22	.01
	(.36)	(.10)	(.34)	(.11)
Education	.12	.12	.12	.12
	(.01)	(.01)	(.01)	(.01)
Education*1973	.02	—	-.01	—
	(.03)		(.03)	
Female	.09	.08	.05	.07
	(.07)	(.06)	(.08)	(.07)
Female*1973	-.08	—	.17	—
	(.20)		(.22)	
Nonwhite	-.12	-.10	-.46	-.43
	(.09)	(.09)	(.10)	(.09)
Nonwhite*1973	.11	—	.22	—
	(.27)		(.28)	
Catholic	-.21	-.20	-.23	-.15
	(.08)	(.07)	(.09)	(.08)
Catholic*1973	.02	—	.66	—
	(.22)		(.26)	
Church attendance	-.14	-.14	-.12	-.12
	(.01)	(.01)	(.02)	(.01)
Threshold 1	.33	.33	.39	.38
Threshold 2	.64	.64	.83	.83
log(L)	-1,629.5	-1,629.8	-1,197.5	-1,201.2
Chi-squared	—	.6	—	7.4
Significance	—	ns	—	ns

Note: Standard errors are in parentheses. Number of cases = 1,476.

reestimating our model for those who have heard but who are below average in education, like those who did not hear. Table 4 presents the results for those who had heard of the decision but had less than 12 years of schooling. The mean education for this group is actually a bit less than for those who had not heard: 8.7 years versus 10.2. The substantive results mirror the findings of Table 2, not Table 3. Even among these less educated respondents, the structural response hypothesis is supported. It appears that the crucial variable is having heard of the Court's decision, not the respondent's level of education.

Discussion

The results presented in Tables 2–4 provide strong evidence of the influence of the Court's actions on the structure of public opinion concerning abortion. In the case of the discretionary scale the result of the decision was to increase the

Table 4. Estimated Abortion Scale Equations for Low Education Respondents Who Have Heard of the *Roe* Decision (Probit Estimates)

	Discretionary Scale Models		Health Scale Models	
Variable	Unconstrained	Constrained	Unconstrained	Constrained
Intercept	−.11 (.21)	−.14 (.17)	.88 (.20)	.91 (.17)
1973 dummy	.09 (.34)	.20 (.08)	.41 (.33)	.28 (.09)
Education	.04 (.02)	.05 (.02)	.10 (.02)	.09 (.02)
Education*1973	.05 (.04)	—	−.02 (.04)	—
Female	.12 (.11)	−.04 (.08)	.04 (.11)	.08 (.09)
Female*1973	−.35 (.16)	—	.08 (.17)	—
Nonwhite	−.16 (.15)	−.22 (.11)	−.55 (.13)	−.53 (.10)
Nonwhite*1973	−.14 (.22)	—	.07 (.21)	—
Catholic	.22 (.13)	.05 (.10)	.09 (.13)	−.03 (.10)
Catholic*1973	−.39 (.20)	—	−.29 (.20)	—
Church attendance	−.11 (.02)	−.11 (.02)	−.11 (.02)	−.11 (.02)
Threshold 1	.31	.31	.46	.46
Threshold 2	.64	.64	.98	.98
log(L)	−962.1	−966.9	−864.1	−865.7
Chi-squared	—	9.6	—	3.2
Significance	—	$p < .05$	—	ns

Note: Standard errors are in parentheses. Number of cases = 871.

Table 5. Predicted Discretionary Scale Response Probabilities

Respondents	Scale Category 0	1	2	3
Characteristics				
White, Catholic, 1972	.51	.11	.10	.28
White, Protestant, 1972	.34	.11	.11	.44
White, Catholic, 1973	.53	.11	.10	.26
White, Protestant, 1973	.28	.10	.11	.51
Nonwhite, Protestant, 1972	.52	.11	.10	.27
Nonwhite, Protestant, 1973	.55	.11	.09	.25
	Differences between Group Response Probabilities			
Contrast Groups				
Catholic-Protestant, 1972	.17	.00	–.01	–.16
Catholic-Protestant, 1973	.25	.01	–.01	–.25
Nonwhite-White, 1972	.18	.00	–.01	–.17
Nonwhite-White, 1973	.27	.01	–.02	–.26

differences between groups. In the case of the health scale, the impact was a general rise in approval for such abortions, with little evidence for structural shift.

The most striking result from Table 2 is the general tendency for group differences to increase in response to the decision. For both nonwhites and Catholics the coefficients become larger in absolute value, indicating greater differences with the contrast group. This raises questions about the role of the Court as a legitimizing agent for policy positions. If the major impact of Court decisions was to bring citizens into agreement on fundamental constitutional issues, our coefficients would have been smaller in absolute value, not larger. The results suggest that the impact of the Court decision was, instead, to crystallize issue preferences further and to lead to greater homogeneity of within-group beliefs.

We can illustrate the changes in group preferences by using the equations in Table 2 to estimate the probabilities that respondents with particular characteristics will adopt particular preferences. We are most interested in the differences between groups and how these differences changed between 1972 and 1973. Table 5 presents the predicted probabilities of each discretionary scale response option for four groups and two years. The top panel gives the predicted probabilities. We are interested in the contrasts between these groups over time. This is presented in the lower panel of the table.[8]

The first row of the lower panel shows the difference between a white Catholic and a white Protestant in 1972. The table shows that the average Catholic was .17 more likely to oppose *all* forms of discretionary abortion (a scale score of zero) than an average Protestant. Similarly the Protestant was about .16 more likely than the Catholic to support all cases of discretionary abortion. Compare these figures with the same differences in 1973, shown in the second row. In 1973 the differences have grown by more than 50%, to about .25 and .25, respectively. This is a rather substantial divergence of the two groups. The third and fourth rows of the panel show that similar divergence occurred between white and nonwhite citizens.

The results presented here are both statistically significant and substantial in their implied effects. The changes shown in Table 5 should allay fears that our findings are statistical curiosities lacking substantive import.

A Theory of Court Impact on Public Opinion

What theory can account for our results? It is apparent that any theory falling into the class of positive response hypotheses will not be able to explain the findings for discretionary abortions. Likewise, a theory predicting greater polarization in every case must fail in light of the results for the health scale. Our results show that sometimes public support for the Court's position increases and sometimes opinions become more polarized.

When public decisions are made—whether by the courts, the president or Congress—the individual's psyche processes the information and comes to some conclusions regarding it. If people were atomistic, the process would stop here. But they are not. Much of the meaning of public decisions rests on their interpretation, and interpretation depends on others. Individuals may react to decisions, but they must also consider the reactions of those about them. In this way they test their own interpretations of what has happened and modify their conclusions in light of the communal interpretations.

So when the Court rules, initial reactions may be entirely individualistic; but within a short period of time the individual is exposed to the reactions of others in the social environment. These environmental inputs may change or reinforce both the interpretation of the public decision and the individual's reactions to it. This process leads to a classic contextual model of political attitudes. As John Sprague (1982) puts it, "Consequences for individual political behavior arise from the embedding of the individual in . . . microenvironments—environments which are likely to be both informationally biased and interpersonally reactive. It is in these politically nonneutral and interactive contexts that political information is received, nurtured, matured and ultimately brought to bear on individual acts of political participation, public policy support, and partisanship" (p. 108).

The informational and evaluative biases of different social groups lead to different patterns of response to public decisions. We assume that members of microenvironments are positively affected by the group norms. Thus the effect of group interaction is to increase agreement with the modal response within the immediate social environment. When between-group variance is high, we would expect group members to move in opposite directions, leading to greater polarization. When between group variance is low, then more uniform shifts in the population are the likely result. (See Sprague 1982 for a detailed account of how such a process might work at the micro level).

In the case of abortion preferences, there was high between-group variance on the discretionary side prior to the Court's decision. Catholics and nonwhites were considerably more opposed to such abortions. On the health scale, however, there was much less between-group variance. Even among Catholics and nonwhites, there was majority support for abortions in cases of threat to the mother's health, rape, or probable birth defects. This is compatible with our finding of increased polarization on the discretionary scale but a uniform increase in support for the health scale. In each case, group members moved in the direction of the group norm.

The nature of the contextual effect also depends upon the homogeneity of opinion within the social environment. If the environment is consensual, the individual is likely to encounter a stream of reinforcements consistently favoring the modal position. If, however, the social environment is heterogeneous, there will not be consistent reinforcement of a single interpretation of events. This will reduce any contextual effects and produce a response more dependent on the individual alone.

We should consider an alternative theory that does not demand the contextual effect we posit. Individual proccesing of information alone could account for our findings if those who were predisposed to favor the ruling came to favor it more while those predisposed against became more opposed. Clearly, this is consistent with the results on the discretionary scale. It is also consistent with the health scale estimates, considering that majorities in all groups favored legal abortions for reasons of health. The psychological process underlying such a result might rest on effective counterarguing with discrepant information and willing acceptance of congenial data.

If both theories explain the same phenomena, Occam's razor would steer us towards the simpler, individualistic, explanation. However, there is a testable difference between the theories, which we take up in the next section.

A Test of the Theory

Because of the post hoc development of our theory to account for the patterns in our earlier data analysis, we cannot consider those results as a test of the theory. We need, therefore, to consider the merits of the theory when confronted with a new and proper test.

The contextual theory clearly predicts that homogeneity in the social environment is the key variable in attitude change in response to court decisions. The theory would predict that those who are more solidly embedded in an environment—who have more of their interactions within the group—are most likely to be affected by the group norms. Those peripheral to the environment should be much less affected.

While the General Social Survey is not rich in contextual information, we do have one opportunity to test this prediction of the theory. As we have seen, Catholics moved further away from non-Catholics on the discretionary abortion scale in the wake of the decision. A clear prediction of the theory would be that Catholics who are most firmly rooted within the Catholic community would be the most affected by the group norms following the decision. While the level of interaction with fellow Catholics can vary with neighborhood and choice of friends and acquaintances, it is clear that the most intense association with other Catholics is through the church. Those who are highly involved in the church would necessarily have greater contact with others like themselves than those who seldom attend services.

There is an obvious problem here. Catholics who attend most frequently are undoubtedly more devout than those who attend less regularly. Therefore they might be closer to the group norm and thus more strongly opposed to abortion even before the Court ruled. In order to prevent this confounding of the analysis, we include it in the specification of the model.

This is also a chance to test the alternative explanation for our findings, the purely individualistic reaction theory. If reactions to Court decisions depend only on internal processes, increased interaction with others would have no effect. Frequency of attendance would have no effect on adjustments after the decision.

This is a clear difference in predictions between the two theories. However, high-attendance Catholics might still react more vigorously to the decision if they were more extreme before the ruling. If we find that these highly involved Catholics already prior to the Court's ruling were much more strongly opposed to abortion than were less involved Catholics, increased opposition from this group after the fact might be due to purely individual processes. However, if this group was not substantially more strongly opposed prior to the ruling, the contextual theory provides an explanation the individualistic theory does not. In this way we hope to replace Occam's razor with a more modern decision rule.

The new model is much like the previous one, with this exception: where the previous model contained only the variables Catholic and Catholic*1973, the new model adds two new variables: high-attendance Catholic and high-attendance Catholic*1973. High attendance is defined as attendance "nearly every week" or more. This represents the top third of respondents. We can write this fragment of the model as

$$y = \ldots + (\beta_1 + \beta_3 A)\text{Catholic} + (\beta_2 + \beta_4 A)\text{Catholic*1973} + u,$$

where A is a church attendance dummy with one indicating high attendance, and zero lower attendance.

In this specification, β_3 indicates how much high-attendance Catholics differ from less frequent churchgoers prior to the decision. This is crucial to distinguishing the individualistic theory from the contextual theory. A large negative value would be consistent with either theory, and the test would become inconclusive. If, however, the coefficient is small, large effects of attendance after the decision must be due to contextual effects, and we can distinguish between the models. That issue rests on the β_4 coefficient. This indicates how high-attendance Catholics reacted to the decision. The contextual theory predicts that β_4 should be large and negative. The individualistic theory predicts this only if β_3 is also large and negative. Thus a small estimate of β_3 and a large estimate of β_4 is unambiguous evidence for the contextual theory and against the individualistic one. A small value of β_4 is evidence against the contextual explanation, regardless of values of β_3. Finally, if β_3 and β_4 are both large, we cannot distinguish between the theories, and the results would be consistent with either explanation.

Once more we estimate the model using the combined 1972–73 GSS data, using only those who have heard of the decision. The results are presented in Table 6.

The estimated coefficients offer strong support for the contextual model and little support for the individualistic one. For the discretionary scale the estimate of β_3 is considerably less than its standard error (a t-value of .62), indicating that high-attendance Catholics were not significantly more opposed to such abortions prior to the decision. The estimate for β_4 is, in contrast, quite large and negative, with a t-value of 1.94. This accords with the contextual theory's predictions but contradicts the individualistic hypothesis.

The health scale results fall into the familiar pattern of a uniform increase in support ($\alpha_2 = .55$, $t = 2.4$) with no structural change. While high-attendance Catholics were more strongly opposed to abortions for health than lower-attendance Catholics, they did not react differently to the decision. In a consensual environment this is what we would expect: the opinion environments are uniform, and so is reaction.

These results are very important for our argument. They show that in the case we are able to test with these data, the contextual theory's prediction is supported. If the theory had not found support in a proper test like this, we could have claimed only that it accounted for the results of our prior findings. But we have shown that it makes predictions not made by a competing explanation.

Interpretation

Our theory explaining reactions to the decisions of the Supreme Court is equally applicable to other public decisions. It would be highly desirable to test it in other settings. Other settings would provide different, and perhaps better, contextual measures and opportunities.

Actions by the president offer one such opportunity. Reactions to foreign crises have long been known to produce a "rally-round-the-flag" effect that raises presidential approval in the immediate aftermath but is often followed by a more sober—and less favorable—assessment (MacKuen 1983; Mueller 1970). The theory developed here suggests a possible explanation for this. In the immediate aftermath of a crisis most reactions are based on individual information processing alone. The crisis event evokes deeply inculcated feelings of regime support and patriotism, producing a short-term gain in approval of the president's actions. In the aftermath, however, social communication processes become the dominant influence. The reaction to events is modified

Table 6. Estimated Abortion Scale Equations Including High Attendance Catholics Variables (Probit Estimates)

Variables	Discretionary Scale	Health Scale
Intercept	-.52 (.14)	.72 (.15)
1973 dummy	.27 (.21)	.55 (.23)
Education	.11 (.01)	.12 (.01)
Education*1973	-.00 (.02)	-.02 (.02)
Female	.08 (.07)	.07 (.08)
Female*1973	-.16 (.10)	-.05 (.11)
Nonwhite	-.13 (.10)	-.48 (.10)
Nonwhite*1973	-.26 (.14)	-.06 (.15)
Catholic	-.18 (.10)	-.01 (.12)
Catholic*1973	-.11 (.14)	-.29 (.17)
High-Attendance Catholic	-.09 (.14)	-.39 (.15)
High-Attendance Catholic*1973	-.38 (.20)	-.00 (.21)
Church attendance	-.13 (.01)	-.12 (.01)
Threshold 1	.29	.25
Threshold 2	.56	.90
log(L)	-2,646.7	-1,805.2

Note: Standard errors are in parentheses. Number of cases = 2,472.

by the social environment, as a within-group consensus arises as to the interpretation of actions during the crisis. The groups favoring the president interpret events favorably and approval remains the dominant reaction to the crisis within the group. But in groups that are predominantly unfavorable to the president, social communication leads to a reevaluation of the initial response. The result is a lowering of approval as the within-group effects become dominant. The end result is greater polarization between the president's supporters and opponents.

We can also compare our results with those focusing on media's effect on political attitudes. Iyengar and Kinder (1987) say that the media do not "tell people what to think," as a variant of the positive response hypothesis would suggest the media do. Iyengar and Kinder's research indicates that media coverage has a substantial and consequential effect on problem salience, however. We suggest that this may be the first step in the process of public reaction to public decisions. Media coverage increases salience. In the case of mundane news—the sort that Iyengar and Kinder consider—this effect is likely to stop with the individual processing component of the theory (much as they find). If the news is less mundane, however, the increased salience leads to discussion with others, and the contextual part of our theory comes into play. While the simple presentation of news does not lead to attitude change, discussions with others in the environment may. Public reaction, then, depends on news salient enough to provoke discussion. The nature of the reaction depends on both the stimulus and the discussion environment.

Finally, consider the types of reactions that are likely following rulings of the Supreme Court. First, most rulings probably have no effect at all. They are of sufficiently low salience and visibility that most of the public remains unaware of them. We are not concerned with these rulings. We are concerned with the rulings where the Court most needs public support—highly visible and probably highly controversial rulings. There are several likely outcomes, depending on the distribution of group opinion among the public.

If the public homogeneously favors the Court's position prior to the ruling, support will rise across all groups. Similarly, if the populace was uniformly opposed to the Court's position beforehand, support will actually decrease in the aftermath. In both cases, the mechanism is the same: individuals are moving in the direction of their (homogeneous) social context.

Different results occur when public opinion is heterogeneous. In such a case groups differ in their reactions, and greater polarization is the most likely outcome. The net effect on aggregate support is indeterminate. Support will rise or fall depending on the relative sizes of the groups and the relative effects within groups. In such cases we should neither be surprised if the net effect is nil nor mistake this for no effect.

The abortion issue's unusual salience and ability to arouse unusually deep feelings may restrict the range of application of our findings. How generalizable are our results? While the abortion decision is unusual, it is not unique. Other decisions of the Court—such as school prayer, the death penalty, and desegregation—also tap deep feelings and relatively well formed public attitudes. The difference between the abortion case and these others is one of degree, not of kind. In this sense *Roe* was not unique, and we think our results have direct applicability.

Not all of the decisions the Court hands down are like these, however. Many cases that arouse public debate—such as redistricting or the rights of the accused—do not have the same moral overtones, and it would not be surprising if citizens were more willing to be convinced on these issues. While the positive response hypothesis might fare better on issues like these, the evidence is against it. As we noted earlier, previous research has not found much support for the positive response hypothesis even when considering these more prosaic decisions. If the abortion issue is unusual, it is not because the public is more responsive to the Court's rulings on other issues.

We think our findings' limitations have more to do with the salience of Court decisions than with their subject matter. *Roe* is unusual because of the widespread attention it received. In more routine landmark decisions the public rarely approaches the levels of interest and involvement that followed *Roe*. This means that the social interaction process we discuss may never come into play, simply because the decision does not become a topic of discussion at home, church, or neighborhood saloon. In these cases we would not be surprised if the Court had no discernible impact on public attitudes. Our theory is of reaction to salient government decisions; therein lie both its limitations and its strengths.

Conclusions and Implications

We began by considering a role of the Court envisioned by some of its early commentators—the "republican schoolmaster." Our results suggest that citizens do listen to the schoolmaster but they also talk in class. While we found evidence for the persuasive powers of the Court in the case of the health scale, reflected in a general increase in support for the Court's position, we also found that in the case of discretionary abortions the Court only increased differences among groups. In each of these cases it is clear that the Court's action had an impact—that the public responded to its decision. However, the public response is not a simple matter of increased support for the positions taken by the Court majority. This result offers a warning that efforts to consider the effect of the Court—or other institutions—on public preferences must not rely simply on aggregate support for policies as the sole measure of impact.

The effect of decisions on the structure of preferences also suggests that decisions may have consequences not apparent from the aggregate level of support for Court-backed policies. For example, we find that Catholics and nonwhites became considerably more likely to oppose all forms of discretionary abortion following the Court's decision. Even if the aggregate percentage in favor of discretionary abortions remained unchanged, this shift may have been quite important. The concentration of religious and ethnic groups in communities and congressional districts suggests that elected public officials may have found themselves confronted with constituents more strongly committed on the abortion issue than before. As a consequence, representatives from heavily black or Catholic districts may have felt more pressure to support antiabortion measures, such as the Hyde amendment, in the Congress. To the extent that geographic constituencies coincide with demographic groups that are strongly affected by Court decisions, these shifts in public opinion may have exaggerated consequences for elected officials (Jackson and Vinovskis 1983). The effect of *Brown* v. *Board of Education* is an extreme example of such a situation. Southern white opposition naturally coincided with a major geographic bloc of representatives.

We have focused on the immediate impact of the Court's decision on the preferences of the mass public. In so doing, we have shown that the public is responsive to the actions of public officials. However, we should be careful not to construe the impact of the Court too narrowly. We present evidence concerning the influence of a decision on public attitudes; yet the Court's influence is surely not confined to the immediate aftermath of its decisions.

In the months and years since the *Roe* decision was handed down there have been increased levels of debate, group mobilization, and political action (Luker 1984). These activities in turn have no doubt influenced citizens' preferences as well. These effects are also an impact of the Court. While our results are important in their own right, we do not suggest that the Court's influence is properly measured only in the immediate effects on public attitudes. By the same token we would suggest that there is no simple standard of "impact." Efforts to reduce the role of the Court (or any other institution of government) to a single summary of its influence are naive. Instead we consider here a single facet of the problem of assessing the responses of citizens to the actions of institutions.

The theory we have proposed to account for public reaction to the Court's decisions is also applicable to the actions of other institutions. We have suggested how this may be applied to actions of the president and how it may be integrated into research on the media. The fundamental message of the theory is that public responses involve both individual and collective components. The observed responses involve the distribution of group opinions as well as individual attitudes. The salience of the stimulus has a profound effect on how the information is processed. Since group discussion takes time, there is also an implicit dynamic process involved that may produce different reactions at different moments. We hope that such notions will enhance our understanding of the interplay of government decisions and citizen reactions.

Notes

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1. Note that the positive response hypothesis is a special case of the structural response model in which the "structural" change is simply an increase in overall support with no change in group alignments.

2. For a complete discussion on these issues see Mohr 1988, esp. chap. 4. Like many who discuss design issues, Mohr emphasizes the problems with the design we are using; but he also recognizes its potential when randomization is impossible, as it is here.

3. The exact wording for questions on abortion were: "Please tell me whether or not you think it should be possible for a pregnant woman to obtain a legal abortion if

a. there is a strong chance of serious defect in the baby?
b. she is married and does not want any more children?
c. the woman's own health is seriously endangered by the pregnancy?
d. the family has a very low income and cannot afford more children?
e. she became pregnant as a result of rape?
f. she is not married and does not want to marry the man?"

Responses to items a, c, and e comprise the "health" scale; items b, d, and f make up the "discretionary" scale.

4. A glance at the table makes it clear that more people supported legal abortions in 1973 than in 1972. As Uslaner and Weber (1980) point out, however, it would be wrong to attribute this unthinkingly to the Court's action alone. They show that the trend had been toward greater approval of abortions and conclude that this alone accounts for the increase in support. Part of our task is to convince the reader that the effects we find are due to the Court's ruling and not some other plausible explanation, such as trend.

5. Education is measured in years of school. Gender is a dummy variable contrasting females with males. Race is a dummy indicating nonwhites. Religion is a dummy indicator for Catholics. The frequency of church attendance variable ranges from zero (never) to eight (several times a week) with a median of four (about once a month).

6. The church attendance variable is specified with a fixed coefficient because preliminary analysis showed absolutely no tendency for this effect to change following the Court's ruling.

7. This difference is only faintly mirrored in concern over the abortion issue, however. While 94% of those who had heard were able to place themselves on our scale items, 89% of those who had not heard were also able to place themselves. Thus, there appears to be relatively little difference between the groups in their willingness to respond to the abortion preference items.

8. The values of the education, gender, and church attendance variables were set to the mean for

the group in constructing this table. Thus, we are comparing average white Protestants with average white Catholics, for example, rather than hypothetical Protestants and Catholics with the same education, gender, and church attendance. This is necessary because we want to present a realistic picture of group differences rather than illustrate the influence of a single variable, holding all others equal, which is the more common practice.

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STRATEGIC POLITICIANS AND THE DYNAMICS OF U.S. HOUSE ELECTIONS, 1946–86

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Analysis of both district-level and aggregate time-series data from postwar House elections supports the thesis that strategic political elites play a pivotal role in translating national conditions into election results and therefore in holding members of Congress collectively accountable for the government's performance. More high-quality candidates run when prospects appear to favor their party; they also win significantly more votes and victories than other candidates in equivalent circumstances. Thus, strategic career decisions both reflect and enhance national partisan tides. The electoral importance of strategic politicians has grown over time in tandem with the trend toward candidate-centered electoral politics. This has rendered the effects of national forces less automatic, more contingent, thus threatening the capacity of elections to enforce some degree of collective responsibility.

Free, competitive elections are supposed to encourage leaders to govern responsibly because voters can fire them if they do not. But the kind of accountability elections enforce depends on the structure of electoral politics. Where votes are cast to choose a governing party—and only incidently particular legislators, as in most parliamentary systems—enforcement of collective responsibility is unproblematic, and incentives for legislators to pursue successful national policies are unambiguous. Contests that revolve around local candidates and issues to the neglect of national parties and programs, like most present-day House elections, reward individual responsiveness at the expense of collective responsibility. They inspire far more sensitivity to the local implications of policies attacking national problems than to the national problems created by aggressive pursuit of local interests (Mayhew 1974a; Fiorina 1980; Jacobson 1987c). A national legislature composed of people who believe that when it comes to elections, "all politics is local" naturally finds it difficult to act responsibly.

For this reason, any feature of congressional election politics that does enforce some degree of collective accountability deserves special attention. The feature that has attracted the most scholarly attention is how a party's electoral fortunes rise and fall with the economy and the public's appraisal of the administration. The stronger the economy and the more widely approved the president's performance, the more congressional seats the president's party wins. The weaker the economy and the more dissatisfied people are with the administration, the more seats the president's party loses. Variations in the partisan division of congressional seats are thus the consequence of an electorate acting, in aggregate, like V. O.

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Key's "rational god of vengeance and reward" (Abramowitz and Segal 1986; Arcelus and Melzer 1975; Kramer 1971; Lewis-Beck and Rice 1985; Tufte 1975, 1978).

Aggregate retribution implies individual retribution: voters might impose collective responsibility simply by voting their pocketbooks and opinions of the administration. But survey evidence makes it doubtful that the process is this simple. Individual analogs of national economic conditions (change in family income, experience with unemployment) appear to have little or no impact on voting for House candidates in general (Born 1986; Kiewiet 1983), though such results are somewhat stronger when analysis is restricted to districts held by incumbents of the president's party and to election years when the economy's performance was below par (Alford and Hibbing 1982; Hibbing and Alford 1981; Weatherford 1986). More general economic assessments—of business conditions, of the administration's economic policies and performance, of party competence on economic matters—sometimes influence individual voting decisions; though, once again, few relationships remain stable across election years (Abramowitz, Cover, and Norpoth 1984; Fiorina 1981, 1983; Jacobson 1983; Kiewiet 1983).

Evaluations of presidents and presidential candidates influence the House vote somewhat more consistently, but even here exceptions appear. For example, midterm assessments of Johnson and Reagan had a significant influence on House voters, while evaluations of Ford and Carter made little apparent difference (Abramowitz 1984; Fiorina 1981; Jacobson 1983; Jacobson and Kernell 1987; Kernell 1977). The impact of presidential coattails also varies considerably from one election to the next (Calvert and Ferejohn 1983). Some studies have even concluded that Watergate did not have a major impact on voting decisions despite the heavy losses suffered by Republicans in 1974 (Conway and Wyckoff 1980; McLeod, Brown, and Becker 1977; Miller and Glass 1977), though other analyses link the House vote to reactions to Ford's pardon of Nixon (Fiorina 1981; Uslaner and Conway 1985).

Some of the voters, some of the time, then, appear to treat the House election as a referendum on the administration and economy. But the survey findings are sufficiently spotty and inconsistent to leave grave doubts whether referendum voting is adequate to account for the observed aggregate patterns. Thus Samuel Kernell and I have proposed an alternative (though complimentary) explanation. We argue that strategic political elites play a pivotal role in translating national conditions into election results and therefore in holding members of Congress collectively accountable for the government's performance (Jacobson and Kernell 1983).

Strategic Politicians

Our explanation takes as its point of departure the same idiosyncratic features of U.S. elections that make the enforcement of collective responsibility problematic. The executive and legislature are elected separately. Electoral partisanship is muted; most citizens think that they should vote for the "better" person regardless of party. The same surveys that offer but limited evidence of referendum voting offer abundant evidence of candidate-centered voting. What voters know and think of the particular pair of candidates running in the district has a major influence on their decision; and they evaluate House candidates far more often in terms of personal character and services than in terms of parties, policies, or national issues (Jacobson 1987c).

The choice offered locally between candidates is thus the main focus of electoral politics. In contests between House incumbents and challengers, the nature of the choice is shaped largely by the political talents and campaign resources of the challenger (Jacobson 1980, 1985b, 1988). Absent a strong challenge—the most common circumstances—the incumbent controls the campaign agenda, monopolizes media attention, and wins handily regardless of partisan trends. It takes a vigorous, well-funded challenge to offer voters the kind of alternative—a "qualified" candidate raising troublesome issues—that can threaten the incumbent with premature retirement.

Strong challenges do not emerge randomly; their occurrence varies with the prospects of victory. As a group, ambitious, experienced career politicians make the most formidable candidates for Congress. They have the most incentive and opportunity to develop the political skills and connections that produce effective candidacies. They also risk the most in trying to move to higher office, for defeat may retard or end a political career. Thus the best potential candidates will also be most sensitive to the odds on winning and so to conditions that affect the odds.

The availability of money for the campaign is certainly one crucial consideration. Astute career politicians will not enter contests without some promise of sufficient funds. People who control essential campaign resources also deploy them strategically. Regardless of their motives for contributing, they do not invest much in hopeless causes; better odds inspire more generous donations to nonincumbent House candidates with the appropriate views (Jacobson 1980). Among the things potential donors consider is the quality of the candidate. Better candidates attract more money, just as the availability of money attracts better candidates.

Because electoral history teaches potential candidates and contributors that the national economy and the president's standing with the public affect election results, these things help shape perceptions of election odds and thus strategic decisions about running and contributing. When national conditions favor a party, more of its ambitious careerists decide that it is the year to go after a House seat. Promising candidates of the other party are more inclined to wait for a more propitious time. People who control campaign resources provide more to challengers when conditions are expected to help their preferred party, more to incumbents when conditions put it on the defensive. Because the effects of campaign spending are sharply asymmetrical—marginal returns on campaign spending are far higher for challengers than for incumbents in House elections (Jacobson 1985b, 1987a, 1988)—converging offensive and defensive strategies produce a net gain for challengers of the party favored by national conditions.

The collective result of individual strategic decisions is that the party expected to have a good year fields a superior crop of well-financed challengers, while the other party fields more than the usual number of underfinanced amateurs. Strategic decisions skew the choice between pairs of candidates across House districts, so voters need only respond to the local alternatives to reflect, in their aggregate behavior, the national circumstances that shaped elite strategies. Personal economic experiences and evaluations of the president may still influence individual vote decisions, but a pervasive habit of referendum voting is not, in theory, necessary for voters to impose collective responsibility through the ballot box.

Taken to its logical extreme, the argument implies that prophecies about a party's electoral prospects could be purely self-fulfilling. But decisions based on illusion are hardly strategic; national conditions *must* have some independent effect on the outcome for the argument to make sense. And it is not difficult to imagine how national politics could affect voting even in a system of electoral politics dominated by local candidates and campaigns. National issues can be turned into local issues if a smart candidate has the resources to do it. Poor economic performance, for example, offers challengers representing the party out of power a wedge to separate the incumbent from his or her usual supporters. Whether the incumbent suffers real damage from the issue depends, however, on whether the challenger has the means and skill to exploit it. Thus a party will benefit more from advantageous national conditions, the better its challengers; and superior challengers will be more successful when national issues can be turned against incumbents. The interactive combination of exploitable national issues and vigorous challenges is what imposes a degree of collective accountability on members of Congress.

That, in brief, is the argument. For it to be valid—for strategic politicians pursuing their own careers to play an important, if unintentional, role in translating national conditions into aggregate election results—two basic conditions must hold. First, objectively superior candidates must be selective about when to try for the House. Empirically, the proportion of high-quality candidates should increase with the odds on winning; in particular, more high-quality challengers should appear when national conditions favor their party, fewer in contrary circumstances. Second, the presence of high-quality candidates must produce votes and victories beyond what can be explained by the conditions that enticed them to run; that is, experienced challengers must win significantly more votes and victories than inexperienced challengers in equivalent circumstances.

Richard Born (1986) has challenged the thesis on both counts. He argues that because many, if not all, challengers make their commitment to run for office well before the election year, most strategic career decisions cannot be influenced by national conditions during the election year. He also argues that other things being equal, high-quality challengers win no more votes than do inexperienced challengers and so cannot contribute to their party's aggregate performance.[1] Insofar as he is correct on both points, one should find little or no relationship between the relative quality of a party's challengers and how well it does on election day.

To determine whether this is so, one needs to measure candidate quality. I use a simple dichotomous measure of quality—whether the candidate has ever held elective public office of any kind. Candidates who have held elective office are considered high-quality, experienced candidates; the others are not. More elaborate and nuanced measures of candidate quality have been developed (Bond, Covington, and Fleisher 1988; Canon 1985; Krasno and Green 1985), but I stick with this simple dichotomy for several reasons. First, it is objective and noncircular, removing any temptation to find some "quality" criterion that applies to all candidates who do well. Second, the measure's very crudity favors the null hypotheses and so offers a tough test of the Jacobson-Kernell thesis.[2] If such a blunt instrument produces results consistent with that thesis, better measures should only strengthen the case. Finally, prior office is the background information pertaining to quality most reliably mentioned in the sources I draw on for data covering the first half of the postwar era. For this period, background information has to be gleaned primarily from newspapers archived at the Library of Congress and elsewhere; since the 1960s, much of this information has been available in *Congressional Quarterly Weekly Report's* special preelection reviews of all House races.[3]

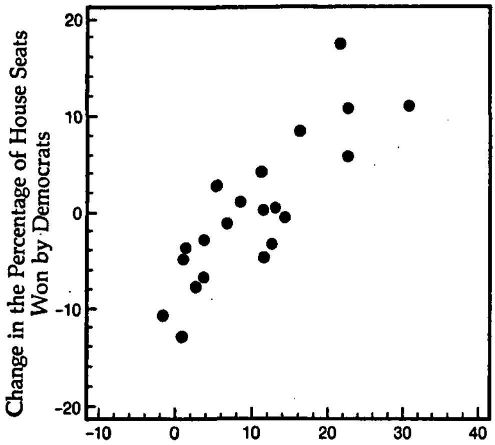
The scatterplots in Figures 1 and 2 are sufficient to show that Born is dead wrong on one or both points. Figure 1 plots the proportion of House seats the Democrats gained or lost in postwar elections against their advantage in percentage of high-quality challengers. The relationship is striking: the correlation between the two variables is .85. Figure 2 takes a slightly different perspective, plotting the net difference in the number of successful challenges (Democrats minus Republicans) in these elections against the net difference in the number of experienced challengers. Again, a remarkably strong relationship emerges; these two variables are correlated at .88. Other formulations yield equivalent pictures; no matter how specified, the relative quality of a party's challengers is closely related to its aggregate electoral performance.[4]

On this evidence, it would be perverse to argue that strategic candidacies are unrelated to election results. The remaining issue concerns the structure of the relationship. Born concedes that "political elites may well predicate personal strategy on their reading of the prevailing partisan winds" (though how they can do so if their decisions are made before the direction of the winds is known is not explained) but argues that "there is little reason to think that the collective consequences of such discrete actions make much difference on where these winds carry the parties on election day" (1986, 610). At issue, then, is the degree to which high-quality candidates merely ride a favorable tide and the degree to which they contribute to the tide by attracting more support, and winning more victories, than would inexperienced candidates in the same circumstances. Before addressing this issue, however, the first task is to confirm that high-quality candidates follow rational career strategies in deciding when to try for the House.

Strategic Career Decisions

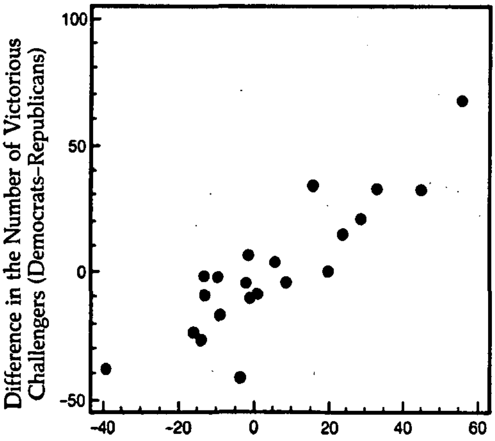
The evidence that high-quality candidates make strategic career decisions

Figure 1. Difference of High-Quality Challengers and Change in Democratic House Seats



Difference in Percentage of High-Quality Challengers (Democrats–Republicans)

Figure 2. Difference of High-Quality Challengers and Difference in Challenger Victories



Difference in the Number of High-Quality Challengers (Democrats–Republicans)

Table 1. The Probability of Victory and the Quality of Nonincumbent Candidates for the U.S. House of Representatives, 1946–86

Type of Race	Number of Cases	Winners (%)	Former Officeholders (%)
Open Seats			
No general election opponent	34	100.0	82.4
Held by candidate's party	821	75.0	62.6
Held by neither party	188	50.0	54.8
Held by opposite party	821	25.0	33.7
Challenges to Incumbents			
Incumbent's vote in last election (%)			
50.0–54.9	1,462	21.1	46.9
55.0–59.9	1,384	8.9	30.1
60.0–64.9	1,207	4.2	21.1
65.0–69.9	953	2.7	16.4
70.0 or more	1,213	.7	11.5

based on the likelihood of advancement is overwhelming. The general point is made in striking fashion by the data in Table 1, which show how tightly the presence of high-quality candidates is linked to the odds on winning, given local circumstances at the time of the election. Open seats attract the largest share of experienced candidates, but within this category, it makes a great deal of difference which party holds the seat because this has such a large effect on the prospect of winning. Unopposed candidates for open seats naturally win every time, and high-quality candidates are most common in these races. Among challengers, the incumbent's margin of victory in the previous election is strongly related to the proportions of both victories and high-quality candidacies. Clearly, career politicians are highly sensitive to local conditions that affect electoral odds (see also Bianco 1984; Bond, Covington, and Fleisher 1985; Canon 1985; Krasno and Green 1988).

Experienced candidates are also sensitive to national conditions that shape electoral odds (though Democrats are far more so than Republicans). The evidence for this is found in Tables 2 and 3. Table 2 presents probit equations estimating the probability that an experienced challenger opposed the incumbent, given local and national conditions, in more than six thousand postwar House elections.[5] Local opportunities expected to influence career moves are measured by the percentage of votes won by the candidate of the challenger's party—and by whether the seat switched party hands—in the previous election. The economy and the president's level of popular approval measure the national conditions. A fifth variable, the party of the administration (scored 1 if Democratic, 0 if Republican), must be entered as a control because of the way some of the other independent variables are scored.

The state of the economy is measured as the percentage change in real disposable income per capita over the year ending in the second quarter of the election year.[6] In using real income growth to proxy the economy, I follow a majority of scholars working in this area, who appear to have two principal reasons for adopting this measure. One is theoretical: it corresponds to the (dubious) assumption that economic voting is about personal finances. The other is that real income growth is usually more highly correlated with election results than any of the likely

Table 2. Estimates of the Probability That the House Challenger Has Held Elective Office, 1946–86

Variable	Democrats (1)	Republicans (2)
Intercept	-1.73*** (.18)	-2.50*** (.17)
Party of administration	-.97*** (.20)	.37* (.22)
Change in party control of seat last election	.58*** (.08)	.52*** (.08)
Votes won by challenger's party in last election (%)	.04*** (.00)	.04*** (.00)
Change in real income per capita (2d quarter)	.04*** (.01)	-.01 (.01)
Presidential approval (2d quarter)	.01*** (.00)	-.00 (.00)
Log likelihood	-1,719	-1,548
Number of cases	3,084	3,123

Note: Coefficients are maximum likelihood estimates; standard errors are in parentheses.

*$p < .05$, one-tailed test.

***$p < .001$, one-tailed test.

alternatives: GNP, inflation, and unemployment. In practice, it is a good summary measure of economic performance on all of these dimensions. Because the administration's party, (not just the Democratic party) is supposed to be rewarded or punished for its management of the economy, this variable is multiplied by −1 when a Republican sits in the White House (this is why the administration dummy is required).

Presidential approval is scored as the mean percentage of citizens approving of the president's performance in Gallup polls taken during the second quarter of the election year.[7] Again, this variable is multiplied by −1 under Republican administrations.

National conditions are measured in the second quarter (April–June) of the election year because this is the period during which most *final* decisions about candidacy must be made. Of course, decisions to run for Congress are sometimes taken months or even years earlier (Born 1986; Maisel 1982)—though two-thirds of nonincumbent House candidates do not register with the Federal Election Commission until March of the election year or later (Wilcox 1987). However, the crucial period is not when individuals decide to run for Congress but when they can no longer gracefully change their minds and decide *not* to run for Congress. Initial decisions to test the political waters may indeed be made right after the last election—if not earlier; final decisions to run or withdraw can be taken right up to the primary. Many potential candidates do decide against running after assessing the incumbent's vulnerability, the availability of funds, and the thrust of national economic and political trends (Jacobson and Kernell 1983).

Table 2, column 1 indicates that all of the local and national conditions significantly affect the probability that a high-quality Democratic candidate challenged

Table 3. Determinants of the Percentage of House Challengers Who Had Held Elective Office, 1946–86

Variable	Democrats (1)	Republicans (2)	Democrats-Republicans (3)
Intercept	54.78***	-10.30	65.09***
	(11.78)	(11.43)	(14.34)
Party of administration	-30.19***	10.63	-40.81***
	(8.28)	(8.05)	(10.10)
Seats won by Democrats last election (%)	-.22	.37*	-.58***
	(.17)	(.17)	(.21)
Change in real income per capita (2d quarter)	1.24**	.11	1.13*
	(.41)	(.41)	(.51)
Presidential approval (2d quarter)	.22*	-.10	.32**
	(.08)	(.08)	(.10)
Adjusted *R*±	.56	.10	.60
Standard error of residuals	4.52	4.38	5.50
Durbin-Watson	1.51	1.72	1.60
Number of cases	21	21	21

Note: The dependent variable is the percentage of challengers who have held elective office; standard errors of regression coefficients are in parentheses.

*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.
***$p < .001$, one-tailed test.

the Republican incumbent. Republican challengers, in contrast, appear to be sensitive only to local circumstances (though the other coefficients show the appropriate signs, and presidential approval does not fall far short of the conventional level of statistical significance).

Aggregate data tell a similar story. In Table 3, the dependent variables are the percentages of Democratic and Republican challengers who have held elective office—and the difference between these two percentages—in postwar House elections. They are regressed on the economic and presidential approval variables, plus the current partisan division of House seats. This variable serves double duty in these equations. It acknowledges that the more seats a party holds, the greater the number of inviting targets it presents to the opposing party, regardless of other circumstances (Oppenheimer, Stimson, and Waterman 1986). But it also serves as a more general measure of opportunity; substantial gains by a party in one election are commonly followed by substantial losses in the next (a majority of which do *not* consist of seats captured from the other party in the previous election), and strategic politicians should take this cycle of "surge and decline" (Campbell 1960) into account. As before, the scoring of other independent variables makes it necessary to control for the party of the administration.

Again, economic conditions and the level of public approval of the president have a significant and substantial impact on the quality of Democratic challengers (Table 3, col. 1). Neither one matters for Republicans, who are sensitive only to the opportunities offered by the current level of Democratic strength in the House (col. 2). However, a composite variable mea-

Table 4. Political Experience and the Frequency of Victory in U.S. House Elections, 1946–86

Type of Race	Number of Cases	Winners (%)[a]
Open Seats		
Incumbent party candidate experienced	347	86.8
Neither candidate experienced	192	76.6
Both candidates experienced	199	65.3
Challenging party candidate experienced	95	49.5
Challenges to Incumbents		
Candidate experienced	1,714	17.7
Candidate not experienced	5,020	4.5

[a]For open seats, percentage of victories for party currently holding the seat.

suring the relative quality of challengers is affected significantly by all of these variables (col. 3). And this is the key variable, because relative quality is what matters on election day (see Figures 1 and 2).[8]

By these measures, Democratic challengers are clearly more "strategic" than Republican challengers. Yet, as we shall see, the quality of Republican challengers has just as strong an impact on election results as does the quality of Democratic challengers. Indeed, the fact that candidate quality is not strictly collinear with economic and other conditions makes it easier to show that it does have an independent influence on House election results.

Candidate Quality and Election Outcomes

Clearly, high-quality candidates run selectively, and their career strategies are sufficiently sensitive to national conditions—as well as other factors affecting their chance of winning—for patterns like those in Figures 1 and 2 to emerge. The remaining task is to show that high-quality candidates contribute to their party's success rather than merely taking shrewd advantage of favorable conditions that would benefit inexperienced candidates just as well.

A necessary, if not sufficient, condition for this to be true is that high-quality candidates win more victories and votes than inexperienced candidates. Table 4 demonstrates that this condition is met. Challengers and candidates for open House seats who have held elective office are much more likely to win. Experienced challengers are almost four times as likely as inexperienced challengers to defeat incumbents. They received an average of 42.2% of the two-party vote (compared to 34.7% for inexperienced challengers) in postwar elections. Candidates for open seats with prior office are also more likely to win, their chances depending also on whether their opponents are similarly experienced (as well, of course, as on which party already held the seat—see Table 1). For (new) open seats held by neither party, experienced candidates win four of five contests with candidates who have never held elective office (N = 59; data not shown).

The success enjoyed by high-quality candidates is not merely a consequence of more careful selection of targets, though such strategic behavior certainly contributes to it. They have a higher probability of winning and win a larger share of votes, even when local and national political circumstances are taken into account. And although high-quality candidates do better in part because they spend more money on the campaign, they receive significantly more votes and are significantly more likely to win even when campaign spending is taken into account. This is evident from results of analyses reported in Tables 5-7.

Table 5 presents probit estimates of the challenger's probability of winning the election, depending on whether the challenger has held elective office and other variables. Column 1, which covers the entire postwar era, shows that both local political circumstances (measured by the vote received by the challenger party's candidate in the previous election) and national tides (summarized by the change in the partisan distribution of the national vote from the previous election to the current one) strongly influence the challenger's probability of winning. It is also evident that high-quality challengers have a

Table 5. Elective Office Experience and the Probability of a Successful Challenge in House Elections

Variable	1946–86 (1)	1972–86 (2)	1972–86 (3)
Intercept	-5.56*** (.23)	-4.12*** (.28)	-3.81*** (.32)
Challenger's party's vote in last election	.09*** (.01)	.06*** (.01)	.03*** (.01)
National shift in two-party vote	.17*** (.01)	.14*** (.02)	.13*** (.02)
Challenger experienced	.45*** (.06)	.46*** (.10)	.34*** (.11)
Challenger's spending	—	—	.79*** (.08)
Challenger's spending squared	—	—	-.06*** (.01)
Incumbent's spending	—	—	-.15* (.08)
Incumbent's spending squared	—	—	.00 (.01)
Log likelihood	-1,219	-433	-347
Number of cases	6,213	2,375	2,375
	Challenger Is Experienced		
Initial Probability			
.01	.03	.03	.02
.05	.12	.12	.10
.10	.20	.20	.17
.15	.28	.28	.24
.20	.35	.35	.31
.25	.41	.41	.37

Note: Coefficients are maximum likelihood estimates; campaign spending is in 100 thousand dollars, adjusted for inflation; standard errors are in parentheses.

*$p < .05$, one-tailed test.

***$p < .001$, one-tailed test.

Table 6. Elective Office Experience and the Challenger's Vote in House Elections

Variable	1946–86 (1)	1972–86 (2)	 (2)
Intercept	10.08* (.36)	12.14* (.58)	15.92* (.50)
Challenger's party's vote in last election	.69* (.01)	.59* (.02)	.41* (.02)
National shift in two-party vote	.94* (.02)	.88* (.04)	.71* (.04)
Challenger experienced	2.79* (.19)	4.44* (.35)	2.41* (.30)
Challenger's spending	—	—	4.62* (.16)
Challenger's spending squared	—	—	−.28* (.02)
Incumbent's spending	—	—	−.46* (.13)
Incumbent's spending squared	—	—	.01 (.01)
R^2	.58	.49	.64
Number of cases	6,213	2,377	2,377

Note: The dependent variable is the challenger's share of the two-party vote; campaign spending is in 100 thousand dollars, adjusted for inflation; standard errors of regression coefficients are in parentheses.

*$p < .001$, one-tailed test.

distinctly greater chance of winning than would be expected from national and local political conditions alone.

An idea of how much difference the presence of a high-quality challenger makes is given by the lower half of the table. In probit analysis, the effect of any one variable depends on the level of probability established by all of the other variables. So, to interpret the results, the first column in the lower section of Table 5 lists an initial probability, and the subsequent columns show how this probability would differ with the addition of a high-quality challenger. The difference is notable. For example, conditions that would give an inexperienced challenger a .10 chance of winning would give a high-quality challenger a .20 probability of winning (compare the columns 1 and 2). The coefficients suggest that experience is worth as much as a 5-percentage-point reduction in the incumbent's previous vote margin (.449/.089 = 5.0) or a 2.6-percentage-point national vote swing to the challenger's party (.449/.174 = 2.6).

Table 5, column 2 shows a similar result for elections since 1972. Column 3 demonstrates that although some of the advantage superior challengers enjoy is attributable to better financing, experience itself continues to make an important independent contribution to the probability of winning. For example, an initial .10 probability of winning would rise to .17 with the addition of a high-quality challenger (compare columns 1 and 4).[9]

In light of these results, it is hardly surprising to find that other things being equal, high-quality challengers also win a larger share of the vote. The evidence is in Table 6, which reports the regression of the challenger's share of the vote on the same independent variables. Taking local

Table 7. National Forces, Strategic Politicians, and Interelection Seat Swings in House Elections, 1946–86

Variable	(1)	(2)	(3)	(4)
Intercept	71.31*** (12.17)	47.17** (14.17)	18.97* (8.25)	43.15** (13.27)
Party of administration	-38.48*** (9.60)	-23.05* (9.76)	—	-21.28* (9.47)
Seats won by Democrats last election (%)	−.87*** (.16)	−.60*** (.15)	−.44** (.13)	−.62*** (.15)
Presidential approval	.31** (.10)	.20* (.09)	—	.19* (.09)
Change in real income per capita	.81* (.41)	.32 (.41)	—	.20 (.38)
Quality of Democratic challengers	—	.33*** (.17)	—	—
Quality of Republican challengers	—	−.52** (.18)	—	—
Difference in quality of challengers	—	—	.62* (.09)	.42** (.13)
Adjusted R^2	.73	.82	.80	.83
Standard error of residuals	3.91	3.16	3.33	3.14
Durbin-Watson	2.16	2.26	2.12	2.31
Number of cases	21	21	21	21

Note: The dependent variable is the change in the percentage of seats won by the Democrats from the previous election; standard errors of regression coefficients are in parentheses.

*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.
***$p < .001$, one-tailed test.

conditions and national trends into account, a high-quality challenger's vote share was, on average, 2.8 percentage points higher than that of an inexperienced challenger in postwar elections (col. 1). Controls for campaign spending in the elections since accurate spending data have become available reduce this figure to 2.4 percentage points. Note that without controls for spending (Table 5, col. 2), experience in elective public office was worth more to challengers (4.4 percentage points) in the more recent period. The electoral value of prior office appears to have increased over these years. I shall return to this important point.

How do these district-level effects add up to aggregate changes in Congress? Some answers are suggested by Table 7. The dependent variable here is the change in the percentage of House seats won by the Democrats from the prior to the current election. I chose to examine changes in seats rather than votes—the other dependent variable commonly examined in this literature—because seats represent the electoral bottom line; in any case, the results are not at all sensitive to this choice, because seat and vote swings are very highly correlated.[10]

Two control variables are again included: the party of the administration (for the same reason as before), and the percentage of seats won by the Democrats last time (in recognition of the reality that the more seats a party already holds, the more difficult it is to win additional seats [Oppenheimer, Stimson, and Watterman

1986]). The other variables are measured as before except that real income change is the average for the election year (this is the time period most commonly used in the literature) and presidential approval is taken from the last Gallup poll before the election. In several presidential election years, Gallup stopped polling as early as June, though most years have readings taken in August or later. This variable is thus measured less precisely than usual; but there is no alternative if we wish to analyze presidential and midterm elections together.[11]

Table 7, column 1 represents the standard referendum model of House elections. As usual, both presidential approval and economic performance have a significant impact on party fortunes; together with the control variables, they explain 73% of the variance in interelection seat swings. Column 2 adds to the analysis the aggregate quality variable for each party's challengers. Both have a strong, statistically significant effect on seat swings in the expected direction, and the variance explained by the equation jumps to 82%. Note that the quality of Republican challengers is at least as strongly related to election results as the quality of Democratic challengers, even though high-quality Republican challengers appear to be less strategic in their behavior than high-quality Democrats. Note also that the effect of real income change is reduced by more than half and ceases to be statistically significant; presidential approval continues to have a significant influence, though it is reduced by a third.

A model that includes the relative quality variable but not income change and presidential approval (Table 7, col. 3) outperforms the model that does the opposite (col. 1). Indeed, fully 80% of the variance in postwar seat shifts can be explained by just two variables: the relative quality of Democratic challengers and the current size of the Democratic contingent.

If, for a more complete specification, presidential approval and income growth are reintroduced (col. 4), the impact of relative candidate quality falls by about one-third but remains substantial. Its coefficient indicates that a 1-percentage-point difference in relative challenger quality is worth almost two House seats; a change of one standard deviation in this variable (8.73) translates into about 16 seats; the difference between its highest and lowest values (32.7) covers 59 seats. Presidential approval continues to make a statistically significant difference, but real income growth does not.

An estimate of the contribution strategic politicians make to interelection seat shifts in specific elections can be calculated by multiplying the regression coefficient in Table 7, column 4 by the difference between relative challenger quality for each election and its 1946–86 average, then dividing by the net share of seats that changed hands. For all postwar elections, an average of about 44% of the seat shifts can be attributed in this way to variations in the relative quality of challengers. In the elections with net shifts of 20 or more seats ($n = 11$), 42% of them could be attributed to strategic politicians. On this evidence, strategic politicians make a major contribution to partisan change in Congress and thus to the electoral enforcement of collective accountability.

Although these equations suggest that the economy has little direct effect on House election results once differences in the quality of challengers are taken into account, skepticism is warranted. Economic conditions must make some real difference, else it would be irrational for strategic politicians to care about the economy. At least, economic conditions should work interactively with relative candidate quality to influence election outcomes (Jacobson 1983, 1986; Jacobson and Kernell 1987). A high-quality candidate should be better able to exploit favorable conditions to undermine the incumbent's support; a party should get more mileage out of favorable conditions with experienced candidates to exploit them. Table 8 presents data that test this possibility by allowing economic conditions and presidential approval levels to interact with aggregate candidate quality.

The results are positive. Real income change has no direct influence but works interactively with relative challenger quality to affect election results (Table 8, col. 1). The same holds for presidential approval levels (col. 2). If both interaction terms are included and the insignificant direct terms are dropped, presidential approval, interacting with candidate quality, continues to make a significant difference; but the interaction term for real income change shrinks to insignificance (col. 3). This evidence is consistent with the view that a party benefits from favorable national conditions only to the degree that superior challengers are available to take advantage of them and that high-quality challengers are more successful when they have favorable national conditions to exploit in their campaigns.

Additional evidence that high-quality challengers contribute significantly to their party's success comes from observing how aggregate interelection vote swings are translated into seat swings. This is commonly measured by the swing ratio, the change in percentage of seats a party wins for a given change in the per-

Table 8. Interactive Models of National Forces, Strategic Politicians, and Interelection Seat Swings in House Elections

Variable	(1)	(2)	(3)
Intercept	22.67** (8.44)	26.95* (14.76)	22.48** (7.09)
Party of administration	-5.31* (2.81)	-9.79 (10.60)	6.51** (2.27)
Seats won by Democrats last election (%)	−.42** (.13)	−.47** (.16)	−.42*** (.11)
Change in real income per capita	.04 (.40)	—	—
Difference in quality of challengers	.34* (.15)	.60*** (.13)	.52** (.16)
Difference in quality of challengers × change in real income per capita	.09* (.04)	—	.04 (.04)
Presidential approval	—	.05 (.11)	—
Difference in quality of challengers × presidential approval	—	.004* (.00)	.003* (.00)
Adjusted R^2	.82	.86	.86
Standard error of residuals	3.16	2.87	2.80
Durbin-Watson	2.35	2.04	2.09
Number of cases	21	21	21

Note: The dependent variable is the change in the percentage of seats won by the Democrats from the previous election; standard errors of regression coefficients are in parentheses.

*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.
***$p < .001$, one-tailed test.

Table 9. Seat Swings As a Function of Vote Swings and the Quality of Challengers in House Elections

Variable	(1)	(2)
Intercept	−.13 (.56)	−1.01 (2.91)
Change in votes won by Democrats from previous election (%)	1.75*** (.14)	1.31*** (.20)
Quality of Democratic challengers	—	.23* (.10)
Quality of Republican challengers	—	−.32* (.14)
Adjusted R^2	.89	.91
Standard error of residual	2.55	2.22
Durbin-Watson	2.19	2.67
Number of cases	21	21

Note: The dependent variable is the change in the percentage of House seats won by the Democrats from the previous election; standard errors of regression coefficients are in parentheses.

$^{*}p < .05$, one-tailed test.
$^{***}p < .001$, one-tailed test.

centage of the national House vote it wins. A standard way to estimate the swing ratio is to regress the seat swing on the vote swing over a series of election years; the regression coefficient gives the swing ratio (Calvert and Ferejohn 1983). Table 9, column 1 estimates a swing ratio of 1.75 for postwar House elections. The fit is quite good (adjusted $R^2 = .89$), but not so good that it cannot be improved by including as independent variables the aggregate quality of each party's challengers (col. 2; adjusted $R^2 = .91$). Controlling for the aggregate vote swing, the better the party's contingent of challengers, the more seats it wins; that is, the better its challengers, the greater advantage a party can take of favorable national tides or the less damage it will suffer from contrary national tides.

Changing Parameters

In the original statement of our theory (Jacobson and Kernell 1983), Kernell and I speculated that the contribution of high-quality challengers to a party's electoral performance had increased over time. In periods of strong party attachments and nationalized electoral competition, presidential coattails would be longer and local outcomes more subject to national forces. Strategic decisions would be even more critical to successful electoral careers, but the quality of individual candidates would be of smaller electoral consequence. The strong postwar trend toward a more candidate-centered style of electoral politics, driven by technological changes and the increasingly successful efforts of House incumbents to carve out personal political franchises, would reduce the electoral importance of national partisan forces while enhancing that of specific candidates and campaigns. In a more candidate-centered electoral politics, the resources and talents of challengers would have a larger impact on district-level results. Elite strategies would thus make a greater independent contribution to a party's electoral performance.

Some findings reported in Table 6 hinted that this sort of change has indeed oc-

Table 10. Interactive Models of the Challenger's Vote and Probability of Victory in House Elections

	Regression (1)	Probit (2)
Intercept	8.41***	-7.99***
	(.82)	(.51)
Challenger's party vote in last election	.80***	.15***
	(.02)	(.01)
National shift in two-party vote	.93***	.18*
	(.02)	(.09)
Challenger experienced	.94**	.40***
	(.39)	(.12)
Year	.19*	.20***
	(.06)	(.04)
Challenger's party's vote in last election × year	-.01***	-.01***
	(.00)	(.00)
Challenger experienced × year	.17***	.00
	(.03)	(.01)
R^2	.59	—
Log likelihood	—	-1,198
Number of cases	6,213	6,213

Note: The dependent variable for column 1 is the challenger's percentage of the two-party vote; its coefficients are regression coefficients; the dependent variable for column 2 is 1 if the challenger won, 0 otherwise; its coefficients are maximum likelihood estimates; standard errors are in parentheses.

*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.
***$p < .001$, one-tailed test.

curred during the postwar era. More direct evidence on the question is found in Table 10. The table presents a probit model equivalent to Table 5, column 1 and a regression model equivalent to Table 6, column 1, but with the addition of a trend term (1946 = 1, 1948 = 2, . . . , 1986 = 21) and interactions between the trend term and the challenger's party's vote in the previous election and the challenger's quality.

Table 10, column 1 indicates that the payoff for quality in terms of votes increased significantly over this period. According to the regression coefficients, its value grew from 1.1 percentage points in the late 1940s to more than 4.5 percentage points in the 1980s. At the same time, continuity between elections has diminished significantly, a sign of the increased volatility of district electorates (another consequence of candidate-centered elections—see Jacobson 1987a). Prior results are also less important than they once were to a challenger's prospects of victory (col. 2); the impact of elective office has increased, but the increase falls well short of statistical significance. Evidently, a high-quality challenge made almost as large a contribution to the probability of success 40 years ago as it does now.

Although the regression and probit findings seem at odds with one another, they can be reconciled by placing them in the context of familiar postwar electoral trends. In the 1940s and 1950s, contests between challengers and incumbents were typically closer than they are today; dis-

Table 11. Interelection Vote Swings and Projected Victories for Challengers in House Elections

Accuracy of Victory Projections	1946–60	1962–70	1972–86
Challengers projected to win who actually won (%)	62.6 (222)	44.7 (114)	31.0 (149)
Winning challengers whose victory was projected (%)	53.5 (260	47.2 (108)	35.6 (171)

Note: Challengers are projected to win if the sum of the national partisan vote swing and the percentage of votes won by their party's candidate in the district in the last election exceeds 50%; the number of cases is in parentheses.

trict vote swings also tracked national vote swings more faithfully. The presence of a high-quality challenger may have had a smaller direct effect on the vote, but in a more competitive electoral environment it was sufficient to raise the probability of victory substantially. Since the 1950s, electoral competition between House incumbents and challengers, as measured by vote margins, has diminished; the vote for the average incumbent has increased from 61% to over 65%, and the number of closely contested "marginal" districts has fallen drastically (Mayhew 1974b). The increment in votes enjoyed by experienced challengers has grown, but so has the distance challengers must make up to overtake the incumbent.

Measured in votes, the increased value of high-quality challenge just about matches the increased value of incumbency. This helps to explain why, despite wider average vote margins, House seats did not become significantly safer between the 1950s and 1970s (Jacobson 1987b). But a successful challenge is now far more contingent on local circumstances—on particular candidates and campaigns—than it once was. Changes in electoral politics have made election outcomes at the district level less subject to national trends, more subject to idiosyncratic local shifts. Consider, for illustration, the hypothetical contribution of national swings to district-level changes. To the degree that challengers' fates are determined by national swings, one should be able to predict the winner by adding the national swing to the share of votes won by the challenger's party's candidate in the district last time and projecting the challenger to win if the sum exceeds 50 percent. Table 11 shows how markedly the accuracy of such projections has declined. For this table, the postwar era is divided into three periods. The first includes elections from 1946 through 1960. The second covers 1962 through 1970, bracketing the transitional decade during which major changes in electoral politics are supposed to have occurred. The third covers elections from 1972 through 1986.

Nearly two-thirds of the projected winners actually did win during the first period; in recent elections, only a third of the projected winners were successful. The national swing projected victories for 54% of the actual winners in the first period; it predicted only 36% of the actual winners in the third period.

Successful challengers in recent elections have had to gain considerably more than their party's average vote swing to win. In the 1940s and 1950s, the average winning challenger needed to add only .1% to the sum of his or her party's district vote last time and the mean national vote swing for the total to exceed 50%; in the 19702 and 19802, the average winning challenger needed an additional 4.4%. In the earlier period, challengers who won when conditions favored their party enjoyed an interelection vote swing only 4.5-percentage-points larger than that of their losing colleagues; in the latest period, the difference is 10.1 percentage points.

Conclusion

Analysis of both aggregate time series and district-level election data from all 21 postwar House elections provides unambiguous support for the idea that strategic politicians have played an important part in translating national conditions into election results. Individual career (and contribution) strategies prompt decisions that enhance the electoral effect of national conditions; in adapting to expected partisan trends, politicians help to realize them.

As the focus of electoral politics has turned increasingly to local candidates and campaigns, the importance of strategic candidacies has, in some important respects, grown. Consequently, the electorate's ability to exercise democratic control by acting as a "rational god of vengeance and reward" has become more contingent and variable, depending to an increasing degree on elite decisions that are only partially determined by the economy and other systematic national forces. National conditions continue to create problems or opportunities for congressional candidates; but how they are handled or exploited makes more difference now than in the past, and this varies among candidates, between parties, and across election years. When a party does not field enough challengers with the resources and skills to take full advantage of the opportunities created by national conditions, partisan swings are dampened (Jacobson 1985a). The danger is that as enforcement becomes less automatic, incentives for collective responsibility—already underwhelming—will diminish further.

Notes

1. Born's principal evidence for this conclusion rests on analysis of individual voting decisions using the five midterm National Election Studies from 1966 through 1982. It is not definitive for several reasons besides the limited number of elections it covers. Several of these surveys seriously underestimate the vote for challengers (Eubank 1985; Eubank and Gow 1983; Gow and Eubank 1984). At least one of them (the 1978 survey) includes a distortingly "low-quality" sample of high-quality challengers (Jacobson 1981a). Born's specification includes, for reasons that remain obscure, the vote margin in the previous election. Theory and evidence (see Tables 1 and 2) indicate that this is strongly correlated with the appearance of a high-quality candidate; no justification is offered for including it in a model of individual voting behavior. Its significant positive effect on the vote in four of these elections is bound to diminish the effect of candidate quality and so to distort the results (Born's n. 13 notwithstanding). Examination of a much more adequate sample of district electorates (an average of 100 respondents in 47 districts) in 1986 showed the strength of the candidacy to have a large effect on the vote under a variety of controls (Jacobson and Kernell 1987). In any case, comprehensive district-level data from the entire set of postwar elections provides a superior basis for testing these hypotheses.

2. By this standard, for example, one of the "high-quality" challengers in 1986 was Harold Stassen.

3. The data were collected under a grant from the National Science Foundation (SES-80-7577). In addition to local newspapers (the Library of Congress has on microfilm at least two newspapers from each state) and all of the *Congressional Quarterly Weekly Report's* reviews of individual House races since they began publishing them (1948–86), background information on candidates comes from *Election Index* (Vernon, VA: Congressional Staff Directory) for 1966, 1968, 1970, 1972, 1974, 1976, 1978, 1980, and 1982; *Biographical Directory of the American Congress 1774–1971* (Washington: GPO, 1971); various regional and occupational editions of *Who's Who;* the *New York Times* (using the obituary index as well as the regular index); various state election pamphlets; and so on. I also checked state yearbooks to determine whether any of the candidates about whom I could find no information had held any state office; for some states, lists of local elected officials were also available for examination. I have assumed that the candidates whose experience remained indeterminate due to lack of information (some highly partisan newspapers did not even deign to mention the names of losing candidates representing the nonfavored party) had not held elective public office. Given their obscurity and that only about a quarter of the *known* challengers have held elective office, the number of errors produced by this

assumption should be quite small; overall accuracy probably exceeds 99%. In any case, all of the substantive results are replicated if the definition of high-quality candidates is narrowed to include only candidates who have held seats in state legislatures or, formerly, the U.S. Congress; for this classification, information is virtually complete and errors minimal.

4. The relationship remains equally strong regardless of whether we measure the variables in percentages or numbers, include total or incumbent-held seats, or take a more restricted definition of high-quality challengers (state legislators or former members of Congress). Here and elsewhere in this paper, I include uncontested seats in computing percentages of high-quality challengers; the results are completely insensitive to this decision.

5. Elections in which the incumbent was unopposed in the current or previous election are omitted; so are at-large elections. Where district lines were redrawn between elections, the previous vote is taken from the incumbent's old district. Results did not change when altered districts were omitted, so they remain in the analysis. In contrast to some of my earlier analyses (Jacobson 1981b, 1987c), I include southern districts for which the requisite data are available. The substantive results are the same whether these districts are included or not.

6. The data to compute quarter-to-quarter changes in real income per capita through 1976 are from U.S. Department of Commerce, Bureau of Economic Analysis (1981). Data for later years are taken from U.S. Department of Commerce, Bureau of Economic Analysis *Survey of Current Business*. Year-to-year changes are computed from data in U.S. Council of Economic Advisors 1987. Quarterly data are not available for 1945–46, so the yearly figure is substituted for that observation for analyses reported in Tables 2 and 3.

7. Presidential approval data are from King and Ragsdale 1988.

8. See also Tables 7–8.

9. In practice, the combination of an experienced challenger and adequate finances defines a high-quality *candidacy*, which is the critical variable in our theory. Unfortunately, accurate campaign spending data for elections prior to 1972 do not exist, and only the challenger's political experience is available to measure the quality of the challenge in earlier elections. Spending enters Table 5, column 3 and Table 6, column 3 in quadratic form in recognition of diminishing returns; for a discussion of functional forms, see Jacobson 1985b. The data have been adjusted for inflation (1986 = 1.00) and are entered in 100-thousand-dollar units for easier interpretation of the coefficients. Expenditure data are from Common Cause 1974, 1976; Federal Election Commission 1977, 1979, 1982, 1983, 1985; and Barone and Ujifusa 1987.

10. See Table 9.

11. In none of the analyses reported here did a significant difference appear between midterm and presidential election years. Combining them also helps to keep the degrees of freedom at a more comfortable level.

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FLOOR BEHAVIOR IN THE U.S. CONGRESS: COMMITTEE POWER UNDER THE OPEN RULE

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The open rule in the House is a complex set of restrictions, limiting, for example, the number of motions and the order of recognition. By incorporating constraints based on the actual set of rules into a model, I show that legislative outcomes appear more predictable than was previously thought. These rules nearly always allow the proponents of legislation to respond to an opponent's amendment, mitigating the potential damage of the latter. An advantage of the approach is that it provides a new interpretation of the changes in floor activity observed in the postreform period.

Even in the wake of the structure-induced equilibrium revolution, the literature on the positive theory of legislatures remains under the spell of McKelvey and his colleagues. While proponents of the new approach have shown that under specialized assumptions, legislative rules lead to stability, floor behavior for most legislation in Congress is typically governed by an open rule, seemingly putting one back in the world of cycling and the absence of equilibrium. Because of the lack of restrictions under the open rule presumed by most models, this view appears to imply that anything can happen on the floor (McKelvey 1976; Schofield 1978; Denzau and Mackay 1983; see also Baron and Ferejohn 1989; Shepsle and Weingast 1987).

In contrast, the literature on committees, particularly that prior to the reforms of the 1970s, focused on stability. Committees expected to win when they brought their bills to the floor, and the evidence suggests they did so. The problem of success under the open rule was seen as largely a problem to be solved in committee. Building the right form of coalition prior to floor consideration was viewed as the primary discretionary variable of committee bill writers. United committees—those that fostered "integration" and had built consensus behind their bills—were almost always successful. As these scholars saw it, behavior under the open rule was well-behaved.

This contrast presents a two-part puzzle to be explained, one part for the theoretical literature and one for the substantive literature. Regarding the first, it is evident that current theoretical models are incapable of explaining the behavior reported again and again in the substantive literature. Regarding the second, the major question that the substantive literature cannot answer is, Given the striking incentives for coalitions to fall apart and reform on the floor, why do they not do so? Neither literature provides an understanding of the "glue" that allows committees and their supporting majorities to stick together on the floor.

I address both parts of the problem

with a single approach. I argue that the anything-can-happen model is unrelated to the open rule used in Congress and that its results, when applied to Congress, are misleading. Understanding floor behavior in Congress and, importantly, how committees maintain their influence over legislation under the open rule, requires modeling the complex set of institutional rules that underpin this rule. Incorporating several salient features of these institutions provides an understanding of the regularities of the floor process.

The model focuses on the implications of a floor contest between two natural opponents, called the majority (M) and minority (m) leaders of the committee. Given the simple model based on restrictions similar to those imposed by the open rule, floor contests of this sort are shown to result in specific outcomes. This follows from the depth limitations that restrict the number of motions on the floor at any one time in combination with the rules of recognition that allow the proponents of legislation to counter amendments offered by opponents by offering perfecting amendments. The first main result shows that when the proponents of legislation expect a floor contest, their best defense is to participate aggressively in the contest by countering all major amendments by opponents that are likely to pass. Counteramendments become additional hurdles over which an opponent's amendment must pass, thereby limiting the potential damage from the latter.

It is apparent, however, that both parties would prefer not to do battle on the floor and instead to come to the floor united behind some compromise that both prefer to the outcome of a floor battle. The problem with this strategy, however, is that, having bargained not to fight on the floor, the minority, m, does better if it reneges by offering amendments to benefit itself (and another majority of legislators) at the expense of the majority, M. The incentives to renege are tempting, for doing so makes m better off than if it had simply initiated a floor contest from the start. This is, once again, the problem of the majority behind the legislation becoming unglued on the floor. The absence of a majority rule equilibrium implies that the incentive and opportunity to renege *always* exist.

Two mechanisms work against reneging. First, future retaliation increases the costs of today's reneging. Since the majority is much less likely to trust the minority in the future, the minority, by reneging, forgoes future benefits of cooperation. Second, there are important institutional features that raise the costs of the minority's reneging. I argue that the leadership plays an important role here. The main theoretical implication is that under the conditions that prevent reneging, committees avoid floor contests altogether and that this reflects a set of equilibrium strategies.

The behavioral implications of the model are remarkably consistent with the empirical literature. First, the model is based on the observation that the most likely—and most difficult—opponents come from within the committee itself. Second, it shows why united or "integrated" committees have a much higher chance of succeeding. Third, it suggests why, in equilibrium, this bargain does not fall apart.

A final implication of the model, explored in greater detail elsewhere (Weingast 1989), concerns the postreform period. The model yields predictions about the implications of the 1970s reforms and provides a new interpretation of the increase in amendment activity reported in the literature (Bach 1986; Smith 1986; Sullivan 1987). The main idea is that the incentives for collusion have broken down in specific ways. The proponents of legislation—now subcommittee leaders—no longer typically expect an absence of threatening amendments on the floor. For those subcommittees where bargaining and collusion have broken down, proponents substitute other strategies to protect their interests, namely, aggressive participation in the amending process.

First I define the phenomenon to be explained, the pattern of committee success on the floor and discuss why this phenomenon requires an explanation. The specific rules, customs, and practices that underpin the open rule in the House are examined and, drawing on some of these, the model is presented. This allows us to solve for amending behavior in floor contests and then to define precisely the problem of a united committee coming to the floor and attempting to remain together. How the congressional institutions help solve the reneging problem is then examined. The implications of the approach for the postreform Congress is developed. The final section then provides some extensions and generalizations.

The Phenomenon To Be Explained

Congress at work is Congress in committees, as Woodrow Wilson's famous phrase had it; and ever since the literature focusing on the influence of committees over legislation has said the same. The main conclusion in this literature is that legislation is written in committee, not on the floor. The development that follows focuses on the period prior to the reforms of the 1970s. While the reforms have led to important changes in floor behavior, these changes can only be understood after modeling behavior in the previous period, and I therefore postpone their consideration until later.

Nearly all works on this topic emphasize that one of the principal tasks of a committee is to build a coalition in support of its legislation and to do so in a way that it survives on the floor. Since Fenno's classic studies of committees, this literature has largely seen floor success as one of integration and consensus (Fenno 1962, 1966, 1973). Integration requires a compromise among legislators with different—and potentially opposing—points of view. When successful, this results in bipartisan support and a united committee on the floor. Furthermore, as one proponent of this view put it, "the available evidence on this topic supports the expectation that the full chamber is reluctant to overturn committees' recommendations" (Rieselbach 1973, 279–80). Regarding the specific question of amendments as threats, Rieselbach notes that these generally

> pose no serious challenge to the provisions of the bill. . . . Chamber norms contribute to this common situation; specialization and reciprocity, taken together, lead to the view that bills should "be written in committee, not on the floor." That is the committee experts are presumed to know best, and where they agree that an amendment should be rejected (or accepted), the rank-and-file legislators are likely to feel constrained to accept this judgement. This premise makes it exceedingly difficult for foes of a bill to attack it at various points in an effort to undermine it by indirection rather than to defeat it by frontal assault. (p. 265)

Evidence of Committee Success

It is worth briefly noting, beyond the voluminous case studies, the type of systematic evidence provided to support the view that committees succeed on the floor. In his book on the appropriations committees, Fenno (1966) reported large amounts of data on the course of appropriations bills as they moved through the legislation process. His evidence concerning floor consideration of the committee's recommendations (a well-integrated, consensus-oriented committee) revealed that the committee typically did well. For example, in studying the House committee's recommendations for 36 bureaus between 1947 and 1962, he found that of the 575 decisions, 89.9% stood without change on the floor. For these bureaus, there were 187 amendments proposed and 74 that passed, reflecting a rate of about 1 amendment per every 8 bureaus per year. For the larger universe of eight departments and public works appropriations, 547 amendments were offered, of which 163, or about 30%, passed during the sixteen years studied. However, Fenno also reports (1966, 453) that of the amendments offered, 24 were proposed by the subcommittee and another 30 were favored by the subcommittee chair, nearly all of which passed. This leaves 112 successful amendments that were opposed by the committee, or less than one successful, opposed amendment per department per year.[1] As departments are large collections of programs, Fenno concludes that "it does not seem that a very great number of amendments were proposed and passed in view of the tens of thousands of recommendations that might be challenged and the equally large number of individual demands for appropriations that surely go unmet" (Fenno 1966, 450).[2]

Dyson and Soule (1970) analyze committee floor success by examining all roll calls between 1955 and 1964 on amendments to committee bills. Their evidence parallels that above, with committees winning about 90% of all roll calls on amendments to their bills. They also find that committee integration makes a difference. Summarizing their empirical findings, they conclude that "the pattern of success in terms of integration is quite clear: *a unified committee is successful about 95 percent of the time*; a Democratic party controlled committee position prevails about 84% of the time; a Republican party controlled committee position prevails about 75% of the time. Minimal partisanship is a key to committee success" (p. 645, emphasis added).

In sum, the principal conclusions of the literature are that committees by and large do well on the floor, that united committees do better than disunited ones, and that successful amendments representing major shifts in legislative goals are relatively rare.

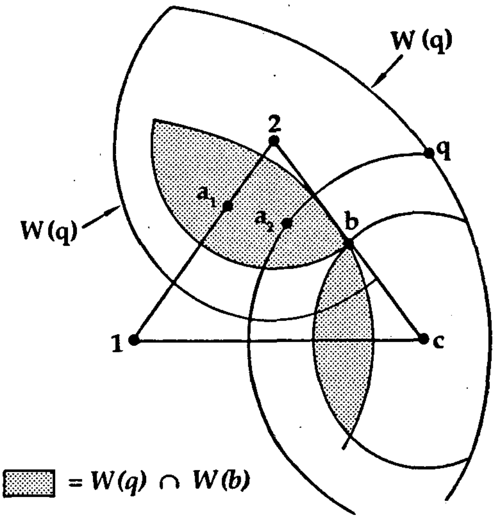
Why This Phenomenon Requires Explanation

To explore a common way of modeling the open rule some technical preliminaries are necessary. The legislature is composed of a set of N agents or legislators ($i = 1, \ldots, N$), choosing over a k-dimension issue space. The issue space is divided into a number of subspaces called jurisdictions, a typical one being m-dimensional. The set of legislators is also divided into subsets called committees, each associated with a specific jurisdiction. I also assume that preferences are separable across jurisdictions so choice can be analyzed in one jurisdiction without explicit reference to the others. For specificity, I focus on a single committee, $C \subset N$, and will call its jurisdiction X, where $X \subset R^m$.

Each agent, i, is endowed with a strictly quasi-concave utility function over X, $U^i(x)$ with an ideal point, x_i, that is, $U^i(x_i) > U^i(x)\ \forall x \neq x_i$. Corresponding to the utility function is the agent's preference relation, P_i, defined as $x P_i y$ if and only if $U^i(x) > U^i(y)$. Legislator i's preferred-to set is defined as the correspondence $P_i(x) = \{y \in X \mid y P_i x\}$. For any policy alternative x, $P_i(x)$ is the set of alternatives preferred by legislator i to x. The win set of x is defined as $W(x) = \{y \in X \mid |y P_i x| \geq |x P_j y|\}$ where $|y P_i x|$ is the number of legislators who prefer y to x. $W(x)$ is the set of alternatives that command a majority against x.

The central result about majority rule holds that except for a very special set of circumstances, $\forall x,\ W(x) \neq \phi$ (see McKelvey 1976; Schofield 1978). In other words, for *any alternative* x, there is a nonempty set of alternatives that command a majority against x. For the model below, this result assures that the relevant

Figure 1. The Committee Gets Rolled by Amendment a_1



win sets I study are nonempty. Central to the analysis is a distinguished point q, the status quo. $W(q) \neq \phi$ implies that for any status quo there exist alternatives that command a majority against it.

The typical approach to modeling the open rule, which I will call the anything-can-happen rule (ACH), is based on the following set of assumptions. First, no change within X can take place unless a proposal is brought forth by C (monopoly gate-keeping power). Proposals by others may only be considered after the committee has brought forth its proposal. Second, once the committee has opened the gates, however, any motion is in order. Third, the amendment process governs voting. This requires that (1) all proposals be made prior to voting and (2) alternatives be voted on in the reverse order to that they were proposed in, with the last motion being voted against the second-to-last, winner against the third-to-last, and so on. Finally, the status quo is the first motion and hence is always voted last. Of these assumptions, the second is the most critical, for it effectively characterizes floor behavior as a free-for-all with no special role afforded the committee once it has opened the gates.

The problem facing the committee under these assumptions can be defined as follows (Denzau and Mackay 1983; Shepsle and Weingast 1987): Since the status quo, q, is voted last, any successful alternative must be in $W(q)$. Given q, the committee initiates consideration by bringing some bill to the floor, $b \in W(q)$. This is depicted in Figure 1 for a three-person legislature with committee ideal labeled C. The assumption that the committee proposal is always voted on just before the status quo implies that any amendment a, offered from the floor must beat both q and b, that is, $a \in W(b) \cap W(q)$. In general, this is not a very restrictive condition, as Figure 1 suggests: it leaves a large number of opportunities for amendment that benefit noncommittee members at the expense of the committee. Thus legislator 2 might offer a_1, which beats both b and q. The problem from the standpoint of the committee is that a_1 makes the committee worse off than q. Once the committee opens the gates under ACH, it cannot assure itself that it will be better off with the new legislation or even as well off as under q. Indeed, a_1 is not a case of committee success but of the committee being "rolled on the floor." This hardly constitutes an explanation for the power of committees within their jurisdiction.

Shepsle and Weingast addressed this problem by analyzing the implications of the "ex post adjustment power" afforded committees via the conference proceeding.[3] However, in their model, floor events do not constrain the committee in conference and hence are, in effect, irrelevant. While the ex post powers endowed by the conference are undoubtedly important, the ability to dominate the conference proceedings probably hinges on success on the floor, as suggested by Krehbiel (1987). Unfortunately, virtually nothing is known about the constraints placed on conference committees based on floor success. However, the effort committees invest in attempting to succeed on the floor underscores that they are important. Floor success, therefore, appears to be a necessary component of committee success. But this begs the question, for we still have no explanation of such success.

Elements of the Institutions Underpinning the Open Rule in the House

The open rule used in the House is not, in fact, open. Rather it is defined by a variety of rules, practices, and customs that considerably restrict the amendment process. A necessary step toward understanding where the ACH model goes wrong is to determine how it departs from the actual rules governing amending activity in the House. What follows describes some of those that appear to have important bearing on floor activity. Since these have not been the focus of detailed study by political scientists, there is only limited discussion in the literature about these rules or about whether actual practice conforms to them.[4]

Debate and amending of bills take place through the Committee of the Whole House. Long-standing custom allows the floor manager of a bill to govern the general debate (Froman 1967, chap. 3; Oleszek 1984, 130). Typically, the committee chair or the chair's delegate serves as floor manager for the majority, while the ranking minority member or a delegate serves for the minority.

The amendment process follows the general debate and is subject to a variety of constraints. Depth limitations concern the structure of the voting tree and limits the number of motions allowed on the floor at any one time. Typically, amendments are considered one at a time. However, for each amendment, the typical situation in the House allows for a perfecting amendment to the amendment, a substitute amendment, and an amendment to the substitute (Deschler and Brown 1982, chap. 27, secs. 6.1, 21; Oleszek 1984, 136–137). Amendments of the third degree (e.g., an amendment to the perfecting amendment to the amendment) are proscribed. Voting typically takes place once the tree for a particular amendment is formed. There is no limit to the number of first-degree amendments that may be made and hence the number of times that this tree can be formed. While voting and motion making can be mixed for any given tree, the Committee of the Whole does not move to the next amendment (and tree) until it has voted on the previous one.

Recognition rules govern who can make motions when. First, committee amendments are treated preferentially, that is, they may be made first and voted on prior to considering amendments of others (Deschler and Brown 1982, chap. 27, sec. 24.1). Second, in recognizing members to make motions, preference is given to committee members over noncommittee members. Third, within the committee, preference is given to seniority. Fourth, recognition alternates between the majority and the minority (Deschler and Brown 1982, chap. 29, sec. 5; Bach 1987).

The recognition rules also distinguish between amendments and perfecting amendments and apply to these two categories in different ways.[5] In effect, two lists are kept. The first applies to first-degree amendments. Once consideration of a first-degree amendment along with all perfecting amendments is completed, these rules specify who is next in line for recognition to make the next first-degree amendment. However, a second list is kept for each new first-degree amendment. For perfecting amendments to each new amendment, the recognition rules begin a new list, giving preference to committee members over nonmembers and to seniority within committee. This advantages committees because it allows them priority to respond to each amendment made by others, and in practice this right is exercised regularly when opponents make threatening amendments (Weingast 1989; see also Oleszek 1984, 136–37).[6]

Another important aspect of amending concerns the device known as an amendment in the nature of a substitute (ANS). While technically an amendment to the bill, an ANS is generally a substitute for the entire bill. An ANS is often specified in the rule and allows the committee to use the substitute bill to be considered as the "original bill for the purposes of amendment" in place of the original bill (Oleszek 1984, 135).

Once the amendment process is completed, the Committee of the Whole rises and reports its "recommendations" to the House.[7] House consideration can be as simple as a single voice vote on the bill, as reported from the Committee of the Whole. But also, roll call votes may be held for each amendment and for the vote on final passage.[8]

By way of summary, the open rule in the House contrasts sharply with that modeled under ACH. The rules do not allow for a free-for-all in which anyone may make motions at any time or in which the order of proposers is random or unpredictable. Instead the rules provide the proponents of legislation a prominent role, allowing them to respond to amendments of their opponents. As I show in the next section, this has surprising implications.

Floor Behavior in Agenda Games

The model that follows draws on some of the important features of the open rule and shows that the latter influence outcomes in specific ways. Inevitably, however, models must be simpler than the world. In focusing on certain aspects of the open rule in the House, therefore, the model abstracts from others: first, it assumes that the contest over the agenda takes place between two sophisticated players, the committee's majority and minority leaders, labeled *M* and *m* respectively.[9] Possible avenues of extending the approach to more proposers are discussed later. Second, it assumes a finite agenda tree, implying a well-defined limit to the number of possible motions.

While these assumptions are made for tractability—the general problem of a floor contest among many sophisticated proposers with a potentially infinite tree is a hard one that cannot be resolved here—they nonetheless draw on key aspects of the congressional environment.[10] The first assumption is rationalized on the grounds that committee leaders hold important advantages over nonmembers by virtue of their expertise and specialization and due to their cooperation with the leadership (e.g., whip polls). They are far better situated to act as strategic proposers in a battle over the agenda than non-committee members.[11] This alone does not guarantee committee success, however, since committees themselves are typically composed of members with diverse views. In practice—at least until the reforms—this diversity of opinion was frequently correlated with majority and minority views, making the committee chair and the ranking minority member frequent opponents in floor battles. Indeed, much of the substantive literature is premised on this natural bifurcation of interests, frequently using *proponents-opponents* as nearly synonymous with *majority-minority* and *Democrat-Republican.*

The second assumption focuses attention on the interaction between the proponents of legislation and an opponent who seeks to alter the bill in a specific manner. The analysis suggests some key aspects of the ability afforded proponents under the rules to defend against opposition amendments.

The Model

Two technical assumptions of the model are that the amendment process governs voting and that all legislators vote sophisticatedly. These greatly simplify the calculations and allow us to calculate the sophisticated winners of an agenda using the intersecting win set algorithm (Shepsle and Weingast 1984).[12]

The series of agenda games studied below are all ones in which the agenda is formed through a specific sequence of proposals. No player is allowed to commit to actions in advance (e.g., to exercise an incredible threat), so the equilibrium concept is *subgame perfect,* defined as follows: Given a history of the game up to move t, h_t, a strategy, s_i, for player i is a rule that specifies a proposal for any move at which i may make one and is a function of h_t, that is, $s_i : h_t \rightarrow X$. An equilibrium is a set of strategies, $s^* = (s_1^*, \ldots, s_n^*)$ such that $U^i(s_i^*/\bar{s}_i^*) \geq U^i(s_i/\bar{s}_i^*)\ \forall\, s_i \neq s_i^*$ and where $\bar{s}_i^*$ is the set of strategies for all players $\neq i$. An equilibrium set of strategies is subgame perfect if the restriction of those strategies to any subgame constitutes an equilibrium in that subgame.

I shall solve the problem of filling out this tree in a series of stages, each an agenda game (AG) of increasing complexity. This approach develops intuition about the strategic problem and shows the effects of a series of tactics in operation together:

AG1. In the first agenda game, M produces a bill, to which m then produces an amendment.

AG2. The second is the same as the first except the majority now may propose a perfecting amendment to the minority's amendment after the latter is announced.

AG3. M also produces a bill, but now, in anticipation of an amendment by m, it also brings forth an amendment in the nature of a substitute.[13] m then chooses whether to propose an amendment, to which, once announced, M may propose a perfecting amendment to m's amendment.

I consider these games separately. At each stage, the outcome of the game is illustrated in a special three-person example, allowing some comparison of the results across the agenda games.

Agenda Game 1. Given a status quo q, the strategy set for M is $W(q)$; given M's choice of $b \in W(q)$, the strategy set for m is $W(q) \cap W(b)$. The game is solved by working backwards through the two stages of the tree. In the second stage, given the history of the game (i.e., q and the bill, b, proposed by M) the problem for m is to choose the amendment, a_1, that is its best response to b, that is,

$$a_1^* = r^m(b,q) \equiv \operatorname*{argmax}_{a \in W(q) \cap W(b)} U^m(a). \tag{1}$$

Given the feasible set from which to choose, $W(q) \cap W(b)$, m optimizes by choosing its most-preferred motion from among those that beat both q and b.

In the first stage, M chooses its bill knowing that it will be amended according to the response function given in relationship 1. Hence, it chooses the bill that leads to the best—or least offensive—amendment. Specifically, the majority solves the problem given by

$$b_1^* = \operatorname*{argmax}_{b \in W(q)} U^M[r^m(b,q)]. \tag{2}$$

The set of strategies given in relationships 1 and 2 are an equilibrium to this agenda game since neither can do better, given the behavior of the other. The equilibrium proposals are (a_1^*, b_1^*), with a_1^* being the bill, as amended, that passes the legislature.[14] This equilibrium is illustrated in Figure 2 for a particular three-person legislature. In this special, three-person case, the amendment is an alternative along the contract locus between M and m. The figure also shows what would happen if M were not to act strategically, and instead simply proposed the point a_1. In this case, m would take advantage of M and propose an amendment, labeled s in the figure (for "sucker"), making M considerably worse off.

Agenda Game 2. However, it is apparent that if M can also make amendments, in this case a perfecting amendment to a_1, it can make itself better off than in AG1. We first define the following notation: Let $F(x,y,\ldots,z) = W(x) \cap W(y) \cap \ldots \cap W(z)$ and allow $F(\bullet)$ to be defined on any number of arguments. Thus, $F(a,b) = W(a) \cap W(b)$, and so on.

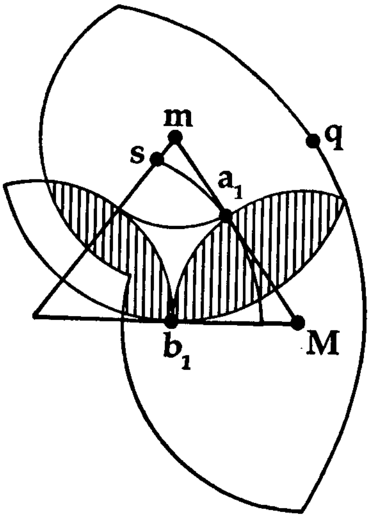
The motion M would make, given the b and a that solve AG1, is

$$aa_2 = R^M(a,b,q) \equiv \underset{aa \in F(a,b,q)}{\operatorname{argmax}} U^M(aa). \qquad (3)$$

In the three-person case illustrated in Figure 2, this is given by aa_2. But the combination (aa_2, a_1^{a}, b_1^*) is not an equilibrium of AG2, since m did not choose a_1^*, anticipating a later perfecting amendment; nor did M choose the original b_1^* in anticipation of this extra strategic variable.

To solve this game, I again work backwards. In stage 3, given a history, q, b, and a, M maximizes $U^M(aa)$ over the feasible set, $F(a,b,q)$, to find the optimal perfecting amendment, aa_2^*. This implies that in stage 2 m now solves the following problem, given b and q: It chooses a_2^* according to

Figure 2. The Solution to AG1



▥ = $W(q) \cap W(b_1)$

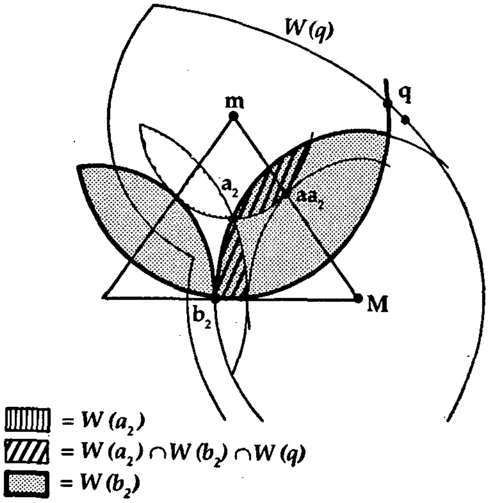
$$a_2^* = r^m(b,q) \equiv \underset{a \in F(b,q)}{\operatorname{argmax}} U^m(R^M[a,b,q]), \qquad (4)$$

where $R^M(a,b,q)$ is defined in relationship 3.

Knowing this, in the first stage, M chooses its bill to maximize $U^M\{R^M[r^m(b,q),b,q]\}$; that is, it picks the bill that gets the amendment that allows it to get its best response, knowing that m will choose according to relationship 4. This yields an optimum bill of b_2^*, so the equilibrium set of proposals for this game is (aa_2^*, a_2^*, b_2^*). For the three-person case, this is illustrated in Figure 3. For this case, M does better in AG2 than AG1, while the reverse holds for m.

Agenda Game 3. In the most interesting strategic case, AG3, in addition to bringing out its bill (stage 1), M now takes advantage of the rules allowing it the ability to make an amendment in the nature of a substitute (S) *prior* to any motion from m (stage 2). The opposition, m, then

Figure 3. The Solution to AG2



makes its amendment (stage 3), and then M makes an amendment to the amendment (stage 4).

Solving AG3 requires again proceeding backwards. At the last stage, given the history of the game thus far ($q,b,s,$ and a) M chooses its perfecting amendment, aa, by optimizing over $F(a,s,b,q)$:

$$aa_3^* = R_2^M(a,s,b,q) \equiv \underset{aa\in F(a,s,b,q)}{\operatorname{argmax}} \; U^M(aa). \tag{5}$$

Knowing this, at the second-to-last stage, m will choose its amendment, a, given the history of the game thus far ($s,b,$ and q) so as to yield its most-preferred perfecting amendment by M, that is,

$$a_3^* = R^m(s,b,q) \equiv \underset{a\in F(s,b,q)}{\operatorname{argmax}} U^m[R_2^M(a,s,b,q)]. \tag{6}$$

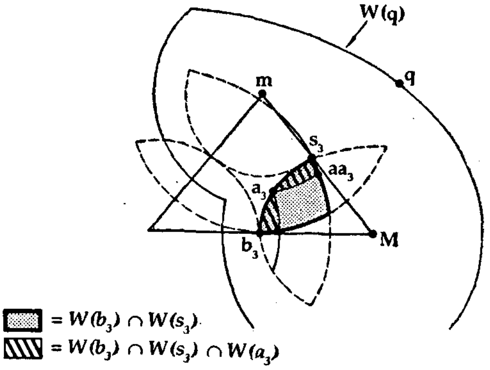
Moving back a stage, M must begin the game by choosing b and then s. It does so knowing that m will next choose according to relationship 6 and that it will respond with a perfecting amendment, aa, according to relationship 5. M will therefore choose b and s according to the following:

$$(b_3^*,s_3^*) = R_1^M(q) \equiv \underset{\substack{b\in W(q)\\ s\in F(b,q)}}{\operatorname{argmax}} U^M\{R_2^M[R^m(s,b,q),s,b,q]\}. \tag{7}$$

This decision rule allows M to choose the optimal b_3 and s_3, given that m responds according to relationship 6 and that it may then respond with the last element of the agenda, aa_3. This yields an equilibrium agenda $(aa_3^*,a_3^*,s_3^*,b_3^*,q)$, where b_3^* is the optimal bill, s_3^* is the amendment in the nature of a substitute brought by M, a_3^* is m's amendment, and aa_3^* is M's amendment to the amendment. This solution is illustrated in Figure 4 for a three-person example. The last motion, aa_3^*, is also the bill, as amended, that passes the legislature.

Taken together, equations 5 and 7 define the optimal strategy for M, given m's strategy, r^m; the latter gives m's optimal response, given the strategy of M; hence they are an equilibrium. Moreover, because they specify optimal choices given any history of previous choices, the equilibrium is subgame perfect. Letting $R^M = (R_1^M, R_2^M)$, we have just shown the following:

Figure 4. The Solution to AG3



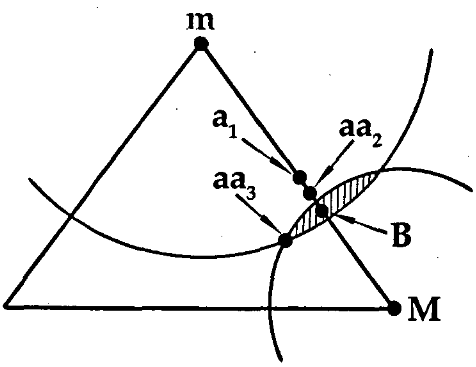
PROPOSITION 1. *The subgame perfect equilibrium of AG3 is given by the strategy pair,* (R^M, r^m), *yielding an equilibrium set of proposals,* $(aa_3^*, a_3^*, s_3^*, b_3^*)$, *and final outcome,* aa_3^*.

In the three-person illustration, *M* does better and *m* does worse in each successive game (see Figure 5).[15] A useful point of comparison of the three models is to evaluate them in the three-person divide-the-dollar setting in which motions are proposals on how to divide the dollar among the players.[16] In this setting, the equilibrium of AG1 is for *M* and *m* to split the dollar. In AG2, *M* garners 66¢ while in AG3, *M* captures 75¢. Here too, we have a progression in which *M* does better and *m* does worse.

The key to understanding this result is that while the majority cannot prevent amendments from being offered or from passing, by participating in the process itself (i.e., anticipating amendments and proposing counteramendments) it can defend its interests and assure itself of gaining on the floor. In the three-person case depicted in Figures 2–4, proponents do quite well. While I have offered no general results (for $n > 3$) about whether AG2 is better for *M* than AG1, I can prove it for an important special case, namely the *n*-person divide-the-dollar game. In this setting, the advantage goes to the person making the last motion, and it can be shown that *M* is always at least as well off in AG2 as *m* is in AG1.[17] Thus, the characterization provided in the three-person illustrations generalizes to an important subset of the general case.

The model suggests that the ability to respond to opposition amendments under the open rule affords proponents of legislation opportunity to defend their interests on the floor. In particular, they are far better off than if floor events went uncontrolled, where they can easily be "rolled" under ACH (as in a_1 in Figure 1).

Figure 5. Comparing the Solutions to AG1, AG2, and AG3



▥ = $P_M(aa_3) \cap P_m(aa_3)$

Endogenous Solution to Reneging

Incentives for Collusion

In the three-person illustration, both *M* and *m* can be made better off than under AG3. To see this, notice that in order to pass, the bill must beat the status quo, that is, $b \in W(q)$. Let the induced-contract locus, ICL(q), between *M* and *m* over $W(q)$ be the set of alternatives such that no other alternative exists where both are better off, that is, $\text{ICL}(q) \equiv \{x \in W(q) \mid \exists \text{ no } y \in W(q), \text{ such that } y\, P_M\, x \text{ and } y\, P_m\, x\}$. In general, the equilibrium of AG3 is not in the set, ICL(q), so that there exist bills that could be brought to the floor, commanding a majority against q, which make both *M* and *m* better off than under AG3, that is, $P_M(aa_3^*) \cap P_m(aa_3^*) \cap W(q) \neq \phi$. Figure 5 illustrates this for the three-person example of Figures 2–4.

This casts the strategic problem facing *M* and *m* in a much different light, transforming it into a bargaining problem between *M* and *m* over $W(q)$, given that either can enforce the alternative aa_3^* if bargaining fails. Since this appears to be a natural setting for noncooperative bargaining theory, I will not provide a specific model of this two-person bargaining game. Instead, I simply assume that the players solve it and call this solution B, where B is on the induced-contract locus between M and m, given $W(q)$.[18] If M and m bring B to the floor and honor the bargain to support B, that is, if they refrain from making amendments, both are better off than under AG3. In this case, the legislature accepts their bill without amendment.

But it is not obvious that both parties can credibly adhere to this bargain. While both are better off colluding than if they play AG3, each side has potential incentives to defect from the bargain on the floor. Consider the majority's problem first. Having brought B to the floor, it can attempt to renege by offering an amendment in the nature of a substitute, $S \in W(B) \cap W(q)$, such that $S\ P_M\ B$. However, if it does so, m is sure to retaliate by offering an amendment. While M may then amend the amendment, this puts the players back in precisely the situation they face in AG3, except now M has begun with less than its optimal first instrument, b_3^*, initiating play instead with B. This results in aa'_3 which, because aa_3^* is the optimal solution for M, cannot be as good for M as aa_3^*, so we have $(B\ P_M\ aa_3^*\ P_M\ aa'_3)$. M is surely worse off if it defects.

The situation facing the minority, however, is different. If it defects, initiating AG3, not only has it forced M to begin with a less-than-optimal bill, but it has usurped M's place in filling out the rest of the tree. The minority now determines the amendment in the nature of a substitute; M may then offer an amendment, but then m may move an amendment to the amendment. This yields as''_3 as the outcome, making m better off than under the AG3 played from the beginning. Under many conditions, m is better off with as''_3 than with B, so it will renege, given the opportunity.

The majority's expectations of whether m will honor the bargain on the floor depends on two things. First, whether m has (short-run) incentives to renege (i.e., whether $aa''_3\ P_m\ B$); second, the incentives for m to honor, induced by the reputational effects from repeat play. In what follows, I assume that the conditions favoring reneging hold and therefore focus on the second factor.

The Effects of Reputation and Repeated Interaction

In general the majority and minority committee leaders expect to play their roles together on legislation with some frequency. The incentives to honor or renege on any one bill, therefore, depend on the effects of this choice on future collaboration. To investigate these effects, I embed the model in a relatively straightforward version of this reputation game (see, e.g., Axelrod 1984 and esp. Kreps 1984) by repeating AG3. I assume that the parties have agreed to bring B to the floor. The majority moves first and must decide whether to trust the minority and bring forth B or not to trust the minority, in which case it initiates AG3 by bringing forth b_3^*. In the former case, the outcome is B; in the latter, aa_3(see Figure 6). Second, if the majority chooses to trust the minority, the latter must decide whether it will honor or renege on the agreement. If it honors, the outcome is B; if it reneges, the outcome is as''_3.

The players have different preferences over the outcomes, as shown in Figure 6. If this game were played only once, m would not honor its agreement, since once M has decided to trust it, m is better off reneging. Because of this, M would not trust m in the one-shot case. As is well known, however, if this game is repeated, m may have countervailing incentives.

Whether these countervailing incentives are sufficient depends on two factors: first the rate at which the players discount future payoffs; second, the set of "trigger" or punishment strategies available to *M* when the game is repeated. These are considered in turn.

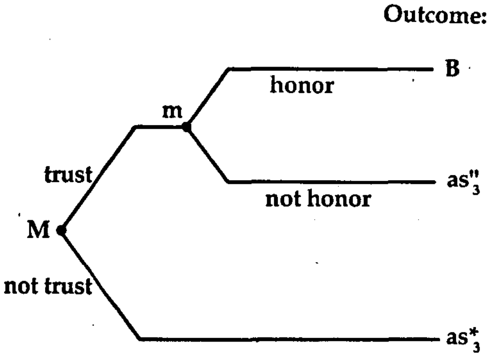
Assume that the parties discount the future by δ, $\delta < 1$. But here I must depart from standard interpretation and decompose δ: in addition to the standard time discount, δ_1 ($\delta_1 < 1$), I must add two factors. The members of today's legislature are not automatically assured of playing in the next session. Since they need to be reelected, the probability of reelection, δ_2 ($\delta_2 < 1$), enters along with the time discount. Second, there are other committees in the legislature, and present committee leaders may anticipate moving elsewhere, possibly breaking off this close and oft-repeated relationship.[19] Let δ_3 be the probability that this relationship is maintained next period, $\delta_3 < 1$. Then $\delta = \delta_1 \times \delta_2 \times \delta_3$.

A trigger strategy for *M* is given as follows: On the first round, *M* trusts *m*. It will trust *m* in future rounds so long as *m* honors this trust. But if ever *m* abuses the trust by reneging, *M* will not trust *m* again. Reneging by *m* thus "triggers" punishment by *M*. This strategy clearly raises the costs to *m* of reneging this one time, for doing so means it forgoes the benefits of cooperation in the future. Whether this forgone benefit is sufficient to deter reneging—and hence whether *M* will play this strategy—depends on how much the value of reneging is relative to the future stream of (higher) benefits.

Assume that *M* plays the above trigger strategy and consider *m*'s choice at the start of the game, that is, at time $t = 0$. Then *m*'s strategy choices reduce to the following: First, *m* may always honor *M*'s trust, yielding the outcome *B* in all periods, and payoff

$$\sum_{t=0}^{\infty} \delta^t U(B). \qquad (8)$$

Figure 6. Reputation and Reneging



Preference Orders

M	*m*
B	aa''_3
aa^*_3	B
aa''_3	aa^*_3

Second, *m* may renege in the first round, and thereafter play AG3 optimally (assuming *M* fails to trust it again; otherwise it will renege again). This yields

$$U(aa'') + \sum_{t=1}^{\infty} \delta^t U(aa^*_3). \qquad (9)$$

m will choose to honor *M*'s trust if and only if expression 8 > expression 9. This depends on whether the benefits forgone from future cooperation exceed the once-and-for-all gains from reneging. This is given by

$$\sum_{t=1}^{\infty} \delta^t U(B) - \sum_{t=1}^{\infty} \delta^t U(aa^*_3) > U(aa'') - U(B)$$

and can be simplified to

$$\frac{[U(B) - U(aa^*)]}{(1-\delta)} > U(aa'') - U(B). \qquad (10)$$

Notice that if *m* has an incentive to renege, it will do so in period 0.

M will play its trigger strategy only if relationship 10 holds, since if it does not hold, M is better off initiating AG3 in all periods, including the first, than it is by playing its trigger strategy. However, assuming that relationship 10 holds and that M plays its trigger strategy, m will honor M's trust. This yields the following:

PROPOSITION 2. *If relationship 10 holds, the strategy pair, M playing its trigger strategy and m always honoring, is an equilibrium to the repeated AG3.*

This discussion shows that a reputation for honoring trust is an important component of assuring that bargains do not become unglued on the floor.[20] But cooperation hinges on relationship 10, and this depends on the discount factor. Only if the future benefits are not discounted too heavily, will cooperation be an equilibrium. Because discounting is based on three factors—δ_1, δ_2 and δ_3—other political considerations beside the time discount affect how future payoffs are weighed and hence whether cooperation can be supported. At times, therefore, these other factors will be sufficiently important to prevent cooperation; so cooperation cannot be universally supported by reputation alone. The next section explores the implications of this conclusion.

Turning to the behavioral literature, there is substantial evidence that reputation and repeat interaction play important roles in a committee leader's ability to defend legislation on the floor. Fenno (1962) emphasizes the importance of stability of leadership (and hence the expectation of repeated interaction). Because the Appropriations Committee is highly stable, "the committee does not suffer from the potentially disruptive consequences of rapid changeovers in its leadership group" (p. 149). Froman suggests that bill managers attempt to foster a reputation for fending off amendments, including those that the committee would benefit by. References throughout this literature to "status" suggest the same thing, since an obvious component of status among a committee leader's peers and potential bargaining partners is the leader's skill at negotiating and defending bargains concerning bills (Fenno 1966; Froman 1967; Hinkley 1978; Rieselbach 1973). Thus, "very few floor managers are willing to see their bills changed on the floor. Even if they are opposed to certain provisions in the bill they may not want amendments to carry. It is a mark of skill and prestige for the committee (and floor manager) if the floor manager can get the bill accepted without many or major changes" (Froman 1967, 77). This is precisely the type of behavior predicted by the model. Moreover, this is consistent with the view of reciprocity commonly discussed here (e.g., Matthews 1960), where reputation plays an important role.

In sum, the main implication of this section is that there appear to be substantial circumstances where it is in the interests of both the proponents of legislation and their most natural opponents, the committee minority, to bargain within committee and to support this bargain on the floor rather than defect.

Implications for the Postreform Congress

Congressional scholars have documented an increase in amending activity from the mid-1960s through the early 1980s (Bach 1986; Hall and Evans 1985; Smith 1986; Sullivan 1987). A natural inference to draw from the increase is that committees have become less successful because their bills are subject to so much more amending. In the words of one scholar, reflecting on the apparent shift in the importance of the floor, "The norms of specialization, committee reciprocity, and apprenticeship met their demise during the 1960s and 1970s and nearly all House committees faced many more floor amendments" (Smith 1986, 5). My approach provides a somewhat different interpretation of these events.

My first point is methodological, namely that one cannot understand the increase in amending activity except in terms of why the previous era was successful at preventing amendments on the floor and what feature of these mechanisms has changed or broken down. In terms of the model, this requires a comparative statics analysis on the equilibrium described above.

One of the major changes of the reform period was the transfer of many of the critical rights over bill writing from 18–22 committees to 125–50 subcommittees. By the time of the Subcommittee Bill of Rights, this change had dramatically altered the ability of the mechanism studied in the last section to enforce bargains. Increasing the number of independent units from 22 to 125 meant that there were now a much larger number of individuals whose reputations became critical for floor behavior. Much less was known about many of them. Previously, committee chairs typically had years of congressional experience prior to chairmanship. The experience (and hence reputation) of many subcommittee chairs was very limited.[21] Moreover, for many subcommittees, both members and leaders were waiting to move on to more attractive ones. These changes imply that members of subcommittees now had, on average, higher discount rates and shorter time horizons, that is, the new subcommittee leaders had a lower δ_2 than their predecessors. Taken together, these factors substantially diminished the incentives provided by the reputation mechanism against reneging. In other words, they imply more floor contests.

The Evidence

Studies of the increase in amending activity have also shown that proponents of legislation are heavily involved in this activity (Bach 1986; Hall and Evans 1985; Smith 1986). This is precisely what the model predicts. The breakdown of the mechanisms underpinning cooperation and united committees means that proponents of legislation have a harder time negotiating and maintaining bargains that keep amendments off the floor, so they now come to the floor expecting amendments. The model predicts that the best defense against threatening amendments is to participate by aggressively offering counteramendments.

Thus, the inference that committees are being rolled more frequently cannot be drawn from the increase in amending activity alone. The reason is that in the face of increased amendment threats, floor managers adapt by bringing a bill to the floor that is a strategic vehicle toward where they want to end up rather than the best they hope to get. In terms of the model, being rolled is an amendment like a_1 in Figure 1, in which the committee is not even a part of the majority coalition that benefits from the bill. This differs considerably from the type of bill-amendment-counteramendment pattern suggested by the model. Participating in an amendment contest in the simple model, AG3, yields the outcome aa_3^*. The loss to the proponents under these circumstances is the difference between the preferred (but unobtainable) negotiated bill, B, and aa_3^*. But this difference is far smaller than the difference between B and the roll-the-committee amendment, a_1.

This approach also yields empirical predictions about patterns of amendments that have not yet been investigated. The predictions are based on disaggregating the pattern of amendments more finely than previous studies, and are of two types. First, my approach suggests that important differences should be observed across subcommittees: (1) all else equal, the more stable a subcommittee in terms of membership and leadership (i.e., the more like a prereform committee), the more likely it is to negotiate and maintain bargains instead of fight on the floor; (2) the experience of the subcommittee leadership should similarly be related to success; (3) subcommittees where the interaction with opponents is more idiosyncratic are likely to be less successful than those with regular opponents; (4) subcommittees whose jurisdiction is fluid, ambiguous, or subject to frequent dispute with other subcommittees should also exhibit less success; (5) finally, holding constant for the importance of an issue, the more active the leadership, the more likely there will be a negotiated bargain.

A second, more readily testable prediction concerns the *sequence* of amendments. As noted above, opponents' amendments that will pass should be countered by either a perfecting amendment or a substitute amendment from the bill's proponents. Thus, the pattern observed in the literature means one thing if one looks solely at the frequency of amendments from opponents and quite another if, as the model predicts, these amendments are typically countered by a committee perfecting amendment that also passed.

The prediction of the response by proponents to amendment threats appears to be borne out (see Weingast 1989). Preliminary results from a sample of amendments under the open rule in the 98th Congress suggest the following picture: While second-degree amendments are not common (89% of amendments have no second-degree amendments), most of these amendments occur precisely where our model predicts. Among the set of amendments opposed by floor managers that eventually pass, fully 68% are met with counteramendments by proponents, and of these, most (80%) pass. Hence most opposition amendments that pass, pass as amended by the proponents (Weingast 1989).

Extensions to the Model: Adding More Opponents

The basic model was highly stylized. In this section I discuss some natural generalizations. The biggest limitation of the model is that it has only two proposers, so it is natural to inquire about generalizing the results. First, notice that the incentives for several parties to bargain remain. However, with the addition of more factions, it may be that the additional faction is not one that regularly interacts with this jurisdiction. Third parties of this type have lower long-run incentives to cooperate and honor agreements. Thus floor fights are more likely between the committee regulars and factions of one-time interests than between parties that regularly interact in a given jurisdiction.

Another special case is worth considering. One rationale for focusing solely on two players from the committee, *M* and *m*, concerned the costs of information. Though committee members are advantaged because of their information about the location of win sets, this does not prevent a strongly motivated legislator off the committee from seeking to make amendments. Less information about win sets has the affect of limiting this legislator's ability to make sophisticated proposals in the sense that he or she cannot easily adjust the proposal to assure it is within the boundaries of the relevant win sets. A natural strategy for such an opponent is to propose a motion that is not near a boundary of a win set (which would risk just missing) but likely to be supported by more than a majority. Moreover, there may be natural focal points for this motion, for instance, the president's proposal. This puts the committee in an interesting strategic position. *M* and *m* now face an opponent on the floor, but one who is not a sophisticated proposer. It will simply move some motion, *A*, known to be in $W(q)$, that commands more than a bare majority support. Since the amendment, *A*, is known in advance, the committee treats this as an additional parameter and attempts to devise motions that will defend against it.

The effect of this type of opponent is threefold. First, the committee always wins. Second, the opponent is not irrelevant, for the threat has forced the committee to adjust its proposed legislation, and it does so in a way that benefits those supporting *A*. Third, the amendment need not be offered to have its desired effect. It is merely the threat that it might be offered, combined with the natural committee reaction to make its legislation beat *A*, that counts, not whether *A* is in fact offered.[22]

Discussion and Conclusion

The simple model developed here potentially provides significant insight into understanding observed floor behavior. The long list of precedents, rules, and customs surrounding the open rule in the House places significant restrictions on the amendment process. I suggest that these lead to a well behaved interaction that does not "wander" anywhere. An investigation of the actual restrictions imposed by the open rule revealed (1) constraints on the number of motions that can be on the floor at any one time, and (2) rules governing who can make motions when. The latter afford committee members the opportunity to respond to opposition amendments with counteramendments. These and other restrictions prevent the process from getting away from the committee. In contrast, under the ACH, the committee plays no necessary role once it has opened the gates. Put simply, the model suggests that when the proponents expect a floor fight, the best defense against amendment threats is to "fight fire with fire," that is, to participate with counteramendments of their own (Weingast 1989).

Incentives for Committee Integration

The model provides an approach to why—at least prior to the 1970s reforms—there appeared to be so few floor fights in which committees were rolled on the floor. There are strong incentives for the natural leaders of these floor battles to bargain and avoid such fights. In devising a bill, committee leaders must do more than produce a bill that benefits a majority. They must produce one sufficiently attractive to the members of this majority to make them willing to support the bill on the floor and forego their opportunities to make amendments. The results derived closely parallel the primary means discovered by substantive scholars for how committees succeed on the floor—by building a coalition in favor of the bill that will support the bill on the floor. What the model adds is an identification of some of the circumstances necessary for successful integration. It also suggests why committee initiatives are more likely to succeed when they are bipartisan bargains brought by a united committee. To the extent that when the proponents of legislation lose on the floor, they lose because of a battle with the committee minority (or with some group that is colluding with the minority), a bipartisan bill supported by a united committee means that the most likely leaders of floor battles have agreed to support the bargain and that they have substantial incentives not to defect from this bargain on the floor. The key point is that integrated committees are those in which bargains will be honored because the players have an incentive not to defect.

This interpretation accords well with Fenno's (1962) classic study. He defined integration "as the degree to which a committee is able to minimize conflict among its roles and its subgroups by heading off or resolving the conflicts that arise" (p. 140). The "acid test of internal integration is [the committee's] capacity to make collective decisions without flying apart in the process" (p. 149). Once the bargain is struck, the committee members are expected to "stick together." The observations commonly reported in the literature parallel the behavior predicted by the model developed above.

The Leadership

An extension suggested by the model is that the leadership may play an important role in maintaining committee bargains on the floor. One problem implicit in the above discussion is that reputation mechanisms cannot mitigate reneging in all circumstances.[23] Hence members have incentives to devise institutions that supplement the mechanisms described in the sense that the institutions reduce the incentive to renege in precisely those situations where reputation mechanisms fail. While a full exploration is beyond the scope here, I outline the possibilities. The leadership may facilitate the durability of cooperation by playing the role of a monitor and enforcer of bargains. This might work as follows: At the conference stage, proponents are bound (in a not-too-well-understood way) by floor events. Suppose, however, that when reneging is known to have occurred, the leadership allows the committee more flexibility in conference (e.g., that it not be bound by the relevant floor event). Such a decision rule would reduce reneging because it lowers the benefits from it. By allowing the proponents of the legislation to ignore a successful amendment when the latter passed solely because an opponent reneged, the renegers are stripped of their success and hence have lower incentives to renege. In precisely those circumstances where reputation is insufficient, the leadership can raise the costs of reneging.[24]

This view comports with modern scholarship on the leadership (Ripley 1967; Sinclair 1983) that emphasizes its role in maintaining coalitions, especially its coordination with floor leaders during the consideration of legislation. The hypotheses here suggest a future line of research on the leadership.

Finally, my approach provides a new interpretation of the change in floor activity during the postreform period. The large increase in amending activity on the floor underscores the lack of an explanation of committee success prior to the reform period. This approach suggests that the explanation for the change is to be found in how the reforms undermined the mechanisms supporting the previous pattern. The evolution from 18–22 stable committees to 125–50 more fluid subcommittees dramatically reduced the effectiveness of the previous mechanisms underpinning negotiating and maintaining bargains. Many subcommittees no longer expected the absence of floor contests. The model suggests that under these circumstances, subcommittees substitute defensive amending activity (as needed) for prior negotiation. They are undoubtedly worse off, but not so much as the increase in amending activity appears to suggest.

The approach developed here addresses the question why there is little defection from committee bills. In brief, the answer provided is twofold: first, the open rule is accompanied by considerable restrictions, making it well-behaved; second, there are countervailing incentives to cycling.

Notes

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1. Fenno also provides evidence of the relative success of amendments favored by the proponents of

the legislation versus those opposed; the former's success rate is over 94% while the latter's is 22% (Fenno 1966, 457 and Tbls. 9.5, 9.6).

2. In his later study, Fenno also provides evidence that divisive committees, lacking cohesion and failing to bring forth legislation with bipartisan support, do much more poorly (Fenno 1973, esp. the discussion of Tbl. 6.16).

3. A second potential explanation is provided by Baron and Ferejohn (1987, 1989). Their important line of research yields a model with an equilibrium under an open rule in which no amendments are made. This occurs because, in effect, the proposer buys off a majority who prefer to end debate rather than wait for more amendments. Their approach will undoubtedly prove useful in addressing the potentially endless number of amendments, something I do not explore.

4. Many books on Congress or committees have discussions of this topic (e.g., Olson 1976; Polsby 1986; and Rieselbach 1973); the same holds for specific works on procedure (Froman 1967; Oleszek 1984). However, these leave unanswered a great many questions about recognition that are critical to understanding agenda processes, as will be shown. For an important departure, see Bach 1987.

5. This is a critical question but unfortunately one on which existing treatments remain ambiguous. An alternative rule would not take into consideration the type of motion being made, simply recognizing more senior members before junior ones.

6. An additional strategic variable available to the committee concerns the limits on amending based on the section or title of the act to which they apply. A judicious use of bundling through titles may affect what types of amendments may be offered. Little appears to be known about this (however, see Humes 1987).

7. A typical rule usually specifies that "at the conclusion of the consideration of the bill for amendment, the Committee [of the Whole] shall rise and report the bill to the House with such amendments as may have been adopted, and the previous question shall be considered as ordered on the bill and amendments thereto to final passage without intervening motion except one motion to recommit" (Froman 1967, 83).

8. Amendments that failed in the Committee of the Whole cannot be voted on in the House; only motions that passed can be reconsidered here. One effect of this rule is that the committee needs to defeat amendments made by opponents of the bill only once, while the opponents need to succeed twice in order for their amendment to be adopted (Froman 1967, 83, 85; Olson 1976, chap. 7; Rieselbach 1973, xx).

9. This is not the same as assuming a two-person legislature and hence one without cycles. It merely assumes that there are two proposers vying for a larger number of legislators. Majority rule cycles still exist.

10. Baron and Ferejohn (1989) have modeled a specific agenda game with a potentially infinite tree. Their approach may provide the avenue for generalizing to the infinite case in other settings.

11. I do not model asymmetric information. This problem has yet to be solved so as to retain the problem of multidimensional cycling and hence is not dealt with directly here. Virtually all models applying asymmetric information are one-dimensional (e.g., Austen-Smith 1986; Gilligan and Krehbiel 1987; Riker and Austen-Smith 1987).

12. As will become apparent from the analysis, the assumption of the amendment process is not necessary, since the results bearing on the equilibrium analysis will extend to more complex agendas (e.g., the results of Banks 1987 show how our results can readily be generalized to the appropriate "two-stage" agenda used in Congress).

13. Note that this use of an "amendment in the nature of a substitute" differs from that in House. Its use here emphasizes a strategic role, even though such a role is not always available in practice.

14. See Austen-Smith 1986, Banks and Gasmi 1985, Baron and Ferejohn 1989, and Shepsle and Weingast 1981 for different proofs.

15. The results from this particular three-person example appear to generalize to any three-person example in two dimensions, though some qualification is necessary for the location of the status quo.

16. See n. 3.

17. A sketch of the proof: At the last stage of AG1, m will search for players whose votes are the cheapest. For n odd, these are the $(n - 1)/2$ voters receiving the lowest payoff offered by M under b. Given this response by m, M maximizes its payoff by dividing the dollar almost equally among a bare majority (that includes it), giving itself just a little less than all others and giving m nothing. m's optimal response is then to give all those given 0 by M ϵ, M just enough to prefer a to b, i.e., $2/(n + 1)$; and itself the rest, approximately $(n - 1)/(n + 1)$. In AG2 on the other hand, M always does better than in AG1. This is easily seen because if M throws away its first move, it is then playing a version of AG1 with itself moving last. This implies that in AG2, it can get at least what m received in AG1, that is, $(n - 1)/(n + 1)$ and possibly more if it uses its first move optimally as the three-person example illustrates.

18. This appears to be a straightforward application of a two-person bargaining model in which M makes the first proposal; m may either accept or reject it (see, e.g., Rubinstein 1982). If m rejects, it may make a counterproposal, and so on. The relevant threat point is aa_3^*. This both seems a good model of this bargaining problem and provides a solution to it.

19. While this factor was probably small or negligible in the prereform Congress, it is critical for the postreform period.

20. Of course, the same line of argument shows that there are many other equilibria (e.g., the folk theorem applies here).

21. Following the reforms, the "subcommittee chairmen . . . usually lacked the experience and political tools of the full committee chairmen of the previous era" (Smith 1986, 6); and "the one subcommittee chairmanship per member rule combined with high membership turnover has resulted in a number of junior and thus inexperienced, subcommittee chairmen" (Sinclair 1983, 25).

22. This suggests a potentially new approach to modeling the influence of the president within the Congress. To the extent that the latter's proposal is always a possible amendment from the floor, the relevant committee must adjust its bill so that it beats the president's proposal. The president is clearly better off from this ability. Notice, however, that the success of this strategy depends on the president's choosing something that is valuable to a large majority of legislators, otherwise it will not be effective.

23. Private information often leads to the breakdown of the incentives for cooperation (see Milgrom, North, and Weingast 1989). Weingast and Marshall (1988) provide a similar argument for the role of institutions in the legislature.

24. This analysis does not explain why leaders might behave in this way. However, if leaders are judged at least in part on their performance in providing services to members, they have incentives to attend to functions of this sort. A promising start here is Calvert 1987a, 1987b.

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POLITICAL TRANSITION, SOCIAL DEVELOPMENT, AND LEGAL MOBILIZATION IN SPAIN

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While it has been frequently asserted that the use of courts is responsive to political and social contexts, the evidence is inconclusive at best. In the case of political context the dearth of evidence may reflect focusing on the U.S. case, where there is relatively limited variation in relevant characteristics. In the case of social context it may reflect a failure to focus on periods of substantial transition. In the present study we examine court usage in Spain between 1960 and 1980. During this period Spain experienced rapid economic growth with its attendant social change and dramatic political transition from authoritarianism to democracy. Both the movement toward democracy and social development are found to be positively linked to increases in legal mobilization.

Over the past three decades, Spain has experienced unprecedented economic growth and dramatic political transition. Responding to prolonged economic stagnation and at the urging of reformist advisors, Franco adopted policies in the late 1950s designed to increase foreign investment and to promote industrialization (Anderson 1970; Gunther 1980; Lieberman 1982, chap. 3, 4; Wright 1977). These policy reforms inaugurated a period of sustained growth dampened only by the oil crises of the 1970s. Between 1960 and 1980 the gross domestic product adjusted for inflation almost tripled. This rapid economic growth produced major alterations in the social structure, which undermined the bases of Francoist support. After forty years of civil war and authoritarian rule Spain emerged with the ratification of the constitution of 1978 as a parliamentary democracy.

The economic growth of Spain and its transition to democracy have been chronicled extensively elsewhere (Alba 1978; Carr and Fusi 1979; Coverdale 1979; Gunther, Sani, and Shabad 1986; Lancaster and Prevost 1985; Maravall 1982; Penniman and Mujal-León 1985; Preston 1986; Share 1986). The present study focuses on the effects of these social and political changes on the use of the legal system for the resolution of private civil disputes. While it has been frequently asserted that the use of the courts is responsive to political and social contexts, evidence on this point is inconclusive at best. The coincidence in Spain of rapid economic growth with its attendant social change and dramatic political transition presents a fortuitous opportunity to reassess the influences of the political and social environments on private legal mobilization.

Literature and Theory

Political Transition

Litigation as politics. Courts have long been treated as political institutions. This treatment has been justified mainly on two grounds. First, at least in developed, Western societies, courts are creatures of the state. Their existence, structure, and jurisdiction are owed to political documents—either constitutions or statutes—and their funding and staffing are to a greater or lesser degree within the influence of political actors. Second, while debates have raged over the legitimacy and capacity of the courts to perform this activity, few scholars would dispute the fact that courts are active participants in the policy-making process. This involvement appears to be true not only for courts in the United States but for courts in other parts of the world as well (McWhinney 1986).

It has been argued more recently that the "politicalness" of courts is far more fundamental to their nature and use than is encompassed in either their structure or their policy-making activities (Grossman et al. 1982; Grossman and Sarat 1975; Jacob 1969; Zemans 1983). From this perspective courts are seen as inherently political because, like other political institutions, they continuously receive demands and authoritatively allocate values (Jacob 1969, 133; McIntosh 1983, 991–92). This function is apparent in public law cases before the U.S. Supreme Court (and other appellate courts) where litigants consciously press for and the Court self-consciously makes public policy. While perhaps less apparent, advocates of this perspective assert that trial courts are similarly called upon to allocate values authoritatively in resolving civil disputes between private parties. The involving of the formal, public authority of the courts to resolve a private dispute is viewed as inescapably part of the political process (Grossman and Sarat 1975, 325). Hence, from this perspective the act of mobilizing the law on one's own behalf even in a private dispute without any conscious political goal can be conceptualized as a form of political participation. Indeed, legal mobilization is seen as a quintessentially democratic form of participation. Through the courts, the private citizen is able to seize directly the coercive power of the state in pursuit of his or her own interests without the problems inherent in representative government. Thus, while not asserting that a legal structure open to private litigants is sufficient to define democracy, this perspective suggests that "the extent to which a citizen is entitled to employ the authority of government by mobilizing the law adds to the diffusion of power and thereby to the democratic nature of the political system" (Zemans 1983, n. 6).

While conceptualizing legal mobilization as political participation, the proponents of this perspective have differed somewhat on its distinctiveness from other forms of participation. Some have argued that the demand for court services arises from different sources from the demands that occur through voting or interest group activity (Jacob 1969, 5; Zemans 1983, 698–99). In contrast, others have argued that although litigation is not the same as orthodox political participation, on close inspection "those differences which at first seem 'obvious' appear to be modest distinctions at best" (Grossman et al. 1982, 87). At a minimum, this perspective would seem to require that legal mobilization not be immune to the general factors conditioning political participation in a society. Whether political norms and structures encourage or discourage participation in the public affairs of a society should influence the rate at which legal services are consumed. If differences in rates of legal mobilization are unrelated to such sweeping differences in the political context, the profitability of conceptualizing litigation as political participation is problematic.

While this approach appears to require some linkage between orthodox political participation and legal mobilization, the structure of that linkage is not specified. Indeed, the literature provides two contradictory expectations. The first is a *generalized* participation hypothesis. This hypothesis predicts that the willingness to use the courts will directly reflect the political context in which they are embedded (Daniels 1982; Grossman et al. 1982; Grossman and Sarat 1971; Grossman and Sarat 1975; Jacob 1969).[1] Where the norms, structures, and process of the political environment place decision making in the hands of a privileged elite, restrict mass participation, and place a high value on order, use of the legal system for dispute resolution is expected to be low. In such politically traditional contexts public expression of conflict is discouraged, and private, informal means of dispute resolution are preferred. In contrast, in more modern political contexts where the norms, structures, and processes encourage mass participation and the public expression of conflict is viewed as legitimate and even desirable, use of formal legal procedures to resolve disputes is expected to be high. Thus, the generalized participation hypothesis predicts that political contexts generating higher levels of orthodox political participation will also experience higher levels of legal mobilization.

The second expectation provided by the literature is an *alternative* participation hypothesis. This hypothesis views litigation as an alternative to orthodox political participation (Grossman and Sarat 1975, 326; McIntosh 1983, 995). In political contexts that restrict or diminish orthodox participation, interests may be expressed through litigation. Thus, this hypothesis predicts that political contexts generating higher levels of orthodox political participation will experience lower levels of legal mobilization.

Available evidence regarding the relationship between the political context and litigation provides neither strong nor consistent support for either hypothesis (Daniels 1982; Grossman and Sarat 1975; Jacob 1969; McIntosh 1983). For example, Grossman and his colleagues (1982) in their study of litigation rates and individuals' decisions to litigate in five U.S. district courts found that the South Carolina district, which they characterized as having a traditionalistic political environment, had the fewest disputes and the smallest percentage of disputes taken to court, as expected. However, the litigation rate in that district was well above the average for all U.S. district courts. They concluded by doubting the efficacy of political context to explain litigation rates: "It may be that 'courts are courts' and what they . . . share in common as participants in the American legal culture is more important than the characteristics of the political environment in which they operate" (Grossman et al. 1982, 113).

Litigation in the political context of Spain. The transition to democracy in Spain provides a critical test of the linkage between political context and legal mobilization. The failure of previous studies to identify a consistent linkage between the political context and legal mobilization may simply reflect the limited variation in political context found within the U.S. case. While the political environment of South Carolina appears to be traditional in contrast to that of California, it appears quite modern in contrast to much of the remainder of the world. A similar argument cannot be made in the case of Spain. The contrast between the Franco regime and the succeeding democratic regime is marked. The nature of the exercise of power varied in different periods of the Franco regime but remained authoritarian throughout (Carr and Fusi 1979). The regime suppressed open political conflict and dissent. Political decision making was confined to an oligarchy largely drawn from the military and leading economic interests. The masses were excluded from participation. Political parties were outlawed, the news media closely regulated, and open elections nonexistent. Mass political participation was confined to the internal structures of the Movement, Franco's political organization. As might be expected, the range of acceptable political alternatives open to consideration within this structure was quite limited. The regime placed priority on the maintenance of social order. While never approaching the elaborate controls of a totalitarian state, the military and particularly the Guardia Civil were effective instruments to control dissent, political opposition, and economic unrest. Thus, the political structure and process of the Franco regime discouraged political participation. Citizens were treated as subjects rather than as participants in the political process. The residual effects of this orientation remain evident. Spaniards who received their political socialization during the Franco regime remain less likely to participate in politics than do individuals who received their socialization during the Second Republic, which preceded Franco, or under the new democratic regime (Lancaster and Lewis-Beck 1986, n. 4).

In contrast the democratic regime has encouraged citizen participation in the process of political decision making and legitimated public conflict over alternative policies. After years of political quiescence Spanish voters have been called to the polls four times for general elections (1977, 1979, 1982, and 1986), three times for local elections (1979, 1983, and 1987), and several times for referenda votes. The latter have included such politically volatile issues as the continued participation of Spain in NATO. The Spanish voter has responded with an average turnout of 74% in national elections and referenda (Lancaster and Lewis-Beck 1986, 653). Free elections have been accompanied by an unrestricted press (Maxwell 1983) and the emergence of vigorous political parties advocating a wide range of political and economic alternatives to the voters (Gunther, Sani, and Shabad 1986; Linz and Montero 1986). Even Franco's implacable foe, the Communist party, has been accepted as a legitimate participant in the political debate. While it maintains a distant veto on some areas of policy, the military has been largely removed from the political process, the jurisdiction of military courts over civil matters has been eliminated in the constitution, and the Guardia Civil has been depoliticized (Boyd and Boyden 1985).

Thus, Spain provides an extreme contrast between a political regime whose norms, processes, and structures discouraged public conflict and participation and a regime whose norms, processes, and structures accept conflict as legitimate and healthy and encourage political participation. The general participation hypothesis would predict that as the Spanish political context became more open to and supportive of participation under democracy, the effects would spill over into the legal sphere producing an increase in legal mobilization. The alternative participation hypothesis would predict legal mobilization to be higher under the Franco regime as interest conflicts were displaced from the political to the legal realm and to be lower during the democratic regime as orthodox pathways to participation became available. If neither of these hypotheses is supported—if this dramatic shift in political context from authoritarianism to democracy in Spain was not accompanied by a change in the use of the legal system to resolve private disputes—it is unlikely that such a linkage will be discovered where the contrasts are less extreme. Moreover, the failure to find a linkage in the presence of such a dramatic shift in the political context would place in question the value of conceptualizing legal mobilization in private disputes as political participation.

Social Development

Social development and litigation. It has been widely assumed that the rate of legal mobilization within a society reflects the level of development of its socioeconomic system (Friedman 1983, 13; Krislov 1983, 182; but see Daniels 1984). This assumption rests on Durkheim's distinction between the mechanical solidarity of premodern societies and the organic solidarity of modern societies (1966). In the former, individuals typically reside in small, rural villages and engage in a subsistence economy of agriculture or fishing. Within this setting they are entangled in a multifaceted web of social and economic relations with others whom they know and to whom they are often related by blood or marriage. The existence of multiple relationships means that the risks associated with the formalizing of a dispute are high. To seek formal resolution of a dispute arising from one dimension of a relationship may place in jeopardy the entire relationship. On the other hand, the multifaceted nature of relationships in such societies provides a basis for informal resolution through mediation. Under these conditions, strong norms develop against the use of formal legal systems for dispute resolution. If informal means of resolving a dispute fail, pressure exists for an offended party to accept a loss rather than disturb the community through formal litigation (Hahm 1969; Kawashima 1973). In contrast, in modern societies a large percentage of the populace resides in urban areas and participates in a highly complex industrial economy. The social fabric of interaction within such societies is far less closely knit than in the premodern society. The nature of the economy requires that transactions occur between individuals who are, in effect, strangers. All that the buyer and seller share in common is the transaction itself. Hence, if a dispute arises over a transaction, the social risks associated with formalizing a dispute are lower and the basis for informal resolution lessened. As Krislov (1983, 162) puts the thesis, "Complexity plus estrangement lead to litigation." In sum, the literature suggests that as a society modernizes, and in particular as it develops economically, the social norms against litigation will decay and the use of formal means to resolve disputes will increase.

Attempts to link social development to increases in litigation, however, have experienced limited success (Blankenburg 1975, 310–16; Daniels 1982, 205; Friedman 1976, 34–35; Friedman 1983, 18–19; Friedman and Percival 1976, 267–302; McIntosh 1980–81; McIntosh 1983, 1003). The most frequently cited contradictory evidence to the straightforward relationship between economic development and increased litigation is Toharia's (1974) study of Spain. Examining litigation rates in civil courts of first instance from 1910 to 1960, Toharia found some oscillation but no consistent pattern of increase. Focusing on the 1960–69 period, litigation rates appeared to remain stable or perhaps even to decline slightly. When these national figures were decomposed to the provincial level, Toharia found a curvilinear relationship. Litigation rates had not increased in the most economically developed provinces or in those that remained rural but had increased in the provinces that were shifting from agriculture to industry (Toharia 1976, 56). A study of litigation rates in U.S. district courts since the turn of the century found a similar nonlinear pattern (Grossman and Sarat 1975, 337). Litigation increased with industrialization except among the most industrialized states. The latter were found neither to have the highest litigation rate nor to have experienced the greatest growth in litigation.

The presence of a nonlinear relationship between socioeconomic development and litigation rates may be explained in at least two ways. First, the mechanisms of the social development argument may hold, but the effects may be offset by other factors effecting litigation. Economic growth and social change increase the number of cases filed, but, given an inelastic supply of courts, this increase at some point begins to yield longer and longer delays between filing and disposition. These higher costs exert a downward pressure on case filings and eventually may negate the positive effects of social development. Second, the effects of social development may be inherently nonlinear. The movement from a rural, agrarian society to an urban, industrial one constitutes a major shift in social structure and norms. Social life amid friends and relatives requires far different behavior than life amid strangers. However, once a society develops sufficiently to undermine the norms against litigation and the potential for informal resolution, additional development may not stimulate increases in litigation. One is as much a stranger in a city of a hundred thousand as in a city of a million. Following this logic, litigation would be expected to increase with social development through the initial transition period as the social structure and norms change but would not be expected to increase with economic growth after the transition.

Either of these explanations is consistent with the finding of a nonlinear relationship between social development and litigation rates. They may also explain the failure to find any relationship. If a study focuses on a time span after the positive effects of development have been offset by the negative effects of increased delay or after the initial transition from an agrarian to an industrial society has occurred, no relationship may appear to exist. For example, studies of economic growth and litigation focusing on Western European countries in this century may miss the effects of development (Blankenburg 1975, 310–16; Friedman 1876, 34–35; Friedman 1983, 18–19). While they have experienced economic growth and development, most of these countries have been modern, urban-industrial states throughout this century.

Social development and litigation in Spain. The Spanish case provides an opportunity to examine the linkage between social development and litigation during a period of rapid economic growth and social transition. Spain throughout this century has lagged far behind most European countries in economic development (Flores 1969; Harrison 1978; Lieberman 1982; Payne 1973; Tamames 1986). While some changes have occurred during the century, they have been far less dramatic than in other countries on the continent. In the sixty years between 1900 and 1960 the percentage of the labor force engaged in agriculture declined only 24 percentage points, from 64% to 40%. As late as 1950 a near majority of the Spanish work force was employed in agriculture (Tamames 1986, 32).[2] In contrast, approximately 40% of the population of the United States and of France was classified as agrarian in 1900. By 1960 the percentages had declined to 7% in the United States and 22% in France (Wright 1977, 3). Despite its major cities and the presence of an industrial base in some areas, Spain in 1960 held as much in common with some underdeveloped Third World nations as it did with its northern neighbors. The economic growth of the 1960s and 1970s transformed the society. Between 1960 and 1980 Spain's national income grew from approximately 40 thousand trillion 1970 pesetas to approximately 90 thousand trillion. By 1980 only 17% of the Spanish labor force was engaged in agriculture (Tamames 1986, 32). The middle class, estimated at 27% of the population in 1950, was estimated to have grown to

42% by 1981 (Fundación FOESSA 1983). Thus, if the linkage between social development and increases in the litigation rate is most likely to occur in the initial period of social transition, we would expect that relationship to be evident in post-1960 Spain.

Data and Methods

The present study focuses on the litigation rate per thousand population in contested civil cases in the Spanish "courts of first instance" (juzgados de primera instancia) between 1960 and 1980.[3] These are trial courts of general civil jurisdiction presided over by a single judge sitting without a jury. They hear virtually all major civil cases. Civil matters of less importance are tried by district courts (juzgados distrito). Despite political transition, the structure of the Spanish legal system has remained virtually unchanged (Lancaster and Giles 1986). Most importantly, the jurisdiction of the courts of first instance has not been altered significantly during the period of interest.[4] Data on case filing were collected from the Judicial Statistics of Spain (*Estadisticas Judiciales de España)*, published annually. Data were collected on total case filings and on selected categories of case terminations including unpaid negotiable instruments, bankruptcy, and cases involving small monetary amounts and large monetary amounts.[5] These are case categories examined by Toharia (1974) and constitute a significant percentage of all cases disposed of by Spanish courts of first instance. For example, in 1980 approximately 80% of all cases terminated by sentence fell into one of these four categories.[6] We expect the effects of social and political context to be registered across the range of case material. If this is not the case—if shifts are only occurring in some areas of litigation—this would suggest the need for a more legally attuned explanation of the changes.

Data were collected both at the national level and for each of the 50 provinces of Spain. We anticipate that if political change does effect litigation rates, these results would be relatively constant across provinces. The expectation regarding the effects of social development is just the opposite. While Spain as a nation has undergone remarkable development, not all regions have participated equally in this process. For example, Catalonia and Madrid have experienced growth in both the industrial and service sectors of the economy, but the poverty in some areas of Andalusia (southern Spain) remains comparable to that in Third World nations. Given that provinces differ in their developmental level, the effects of social development on litigation may differ across provinces. Thus, data at the provincial level may provide additional leverage to assess the effects of the social and political contexts.

The time span of the study is determined partially by the phenomenon of interest and partially by practicality. All available evidence suggests that economic development commenced around 1960. Toharia (1974) has documented an absence of relationship between development and litigation (at least at the national level) prior to 1960. Thus, 1960 appears to be an appropriate starting point for the study. While data on the activities of the courts are compiled and published on an annual basis, there is a significant lag between the initial compilation and the publication. The most recent published data available at the time the study was undertaken were for 1980.

Assessing the effects of the transition to democracy requires determining the point at which the transition occurred. A simple and straightforward solution to this problem is to use 1978, the year when the democratic constitution was approved by the voters. However, the public acceptance of the constitution was the final step in a lengthy political process which contained several significant dates. At least one political historian has argued that the end of the Franco regime began in 1969. By this point there were those within the elite "who were moving toward the conclusion that the dramatic social and economic changes of the decade between 1959 and 1969 had rendered the political structure of Francoism increasingly obsolete" (Preston 1986, 16). This was the year in which Franco realized his growing incapacity and gave effective control of the government to his long-time chief lietenant, Carrero Blanco, and confirmed the succession of Juan Carlos. The period from 1969 to 1975 was one of political turmoil. Many of the elites and functionaries involved in developing industries and those of a generally liberal persuasion began to consider democratic possibilities in anticipation of the eventual demise of Franco. The Catholic Church also clearly moved to distance itself from the Franco government with the effect of undermining the moral legitimacy of the regime. Despite the extensive use of force by the regime, strikes among workers increased in this period, as did protests from students and the terrorists tactics of Basque nationals. The death of Franco in 1975, while by no means assuring the emergence of participatory democracy, constituted a major blow to the recalcitrant falangists and provided an opening to democracy, which was skillfully and successfully pursued by Juan Carlos and the reformist political leadership. The first election to select members of the Cortes that would write the new constitution was held in 1977. While the possibility of military intervention to reverse the process existed,[7] it was clear by this point that the institutional transition to democracy was under way. The next years saw the writing and ratification of the constitution and the realization of institutional reforms such as the creation of the Constitutional Court. The end of the critical period of transition may have occurred as late as the election of 1983, with the peaceful ascension to power of the Socialist party.

The transition to democracy in Spain is probably best conceptualized as a process commencing around 1969 and continuing through the study period. The process, however, was not monotonic or continuous proceeding rather by periods of surge. This process will be operationalized as a system of dummy variables. The 1960–68 period will serve as the basis for comparison (i.e., suppressed category). Dummy variables will be constructed to designate three time periods; 1969–74, 1975–76, and 1977–80. These dummy variables provide a rough surrogate, at best, for the process of democratization, marking as they do major periods in the transition. But, given the differences between the political context in Spain in the 1960–68 period and in the 1977–80 period, if a more democratic political context effects litigation, it should be reflected in the contrast between the litigation rates in these periods.

Social development will be measured using two alternative indicators. The first is gross domestic product (GDP), computed in millions of constant 1970 pesetas. GDP has several advantages as a measure of development. First, it is the broadest indicator of a country's economic activity. Therefore, GDP captures the essence of the economic growth of a country as a whole. Second, because of its breadth, GDP is the accepted standard for measuring economic growth on a comparative and international basis. Third, alternative indicators of development, such as the percentage of the labor force in industry or in agriculture, do not capture the nature of Spanish economic development in the 1960s and 1970s. Spain has a service sector heavier than either Germany, Britain, or France. Much of the 1960s economic growth was consumer-driven, with the growing service sector providing the fuel. Fourth, GDP is readily available on an annual basis at the national level and on a biannual basis for the provinces.

Data on the GDP were collected from reports by the Banco de Bilbao. At the provincial level data for the unreported years were estimated by interpolation. The provincial estimates were adjusted so that their sum was consistent with the aggregate national data for the given year.

The second indicator of development employed is the percentage of the total GDP attributable to the industrial and service sectors of the economy. A shortcoming of GDP as an indicator of development is that it is insensitive to the distribution of activity among sectors of the economy. Hence, an increase in GDP may reflect increased production in the modern sectors of the economy (i.e., industrial and service) or in the traditional sectors (i.e., agricultural). In contrast, percentage of GDP attributable to industry and service provides a direct measure of the distribution of economic activity between the modern and the traditional elements of the economy.

While conceptually distinct, these two indicators are empirically linked. The simple correlation between GDP and the percentage of GDP attributable to the modern sectors is .97. Since the percentage of the GDP attributable to the modern sectors of the economy was only collected at the national level, it is not employed in the provincial-level analysis. However, given the strong relationship between the two indicators and the consistent findings at the national level regardless of which indicator is employed, this does not appear to be a serious problem.

While the overall structure of the court system remained constant throughout the study period, the number of courts of first instance did fluctuate. From 1960 through 1965 there were 576 courts of first instance. The number decreased each subsequent year until 1972, reaching a low of 465 in that year. This pattern was reversed between 1972 and 1980. In the latter year 518 courts of first instance were in operation.[8] An increase in the number of courts would be expected to decrease delay and hence increase the litigation rate. Alternatively, a decrease in the number of courts would be expected to increase delay and decrease the litigation rate. To control for the effect of court availability on legal mobilization, the number of courts will be included in the analysis.

Findings

Given that rapid economic growth and social transition occurred between 1960 and 1980, did the litigation rate increase as hypothesized in the social development model? This question can be directly assessed in Figure 1, where the litigation rate in all contested civil cases is arrayed by year. Toharia (1974) examined the 1960–69 subset of this figure and concluded that litigation was unrelated to social development. Viewed in the full time span of 21 years, however, it is apparent that the litigation rate did increase substantially during this period of economic growth. Moreover, the increase was not the result of growth in one or two types of cases. Between 1960 and 1980 the litigation rate for bankruptcies increased from less than .0025 per thousand to over .0175 per thousand. The litigation rate for cases involving unpaid negotiable instruments increased from approximately .4 to 2.0 per

Figure 1. Contested Civil Cases per 1,000 Population by Year

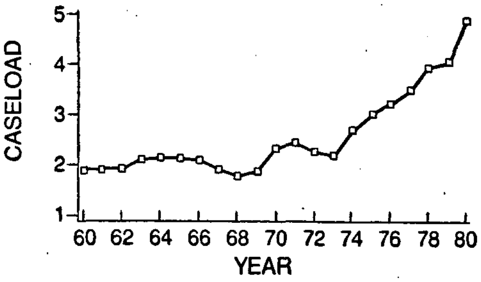


Table 1. Regression of Spanish National Litigation Rates, 1960–80

Variables	Equation 1 (All Years)	Equation 2 (1980 Omitted)	Equation 3 (All Years)	Equation 4 (1980 Omitted)
GDP	$.09 \times 10^{-5}$***	$.07 \times 10^{-5}$***	—	—
Percentage of GDP due to service and industry	—	—	16.53**	11.52**
1969–74	.29	.29*	.62**	.53**
1975–76	.43	.58**	1.01***	1.05***
1977–80	.88*	.94***	1.38***	1.39***
Number of courts	.01**	.01***	.01**	.01
Intercept	-5.12**	-3.46**	-16.90**	-11.21**
R^2	.92	.95	.92	.94
Mean standard error	.24	.15	.25	.17
Durbin-Watson statistic	1.65	2.01	1.70	2.03

*$p \le .10$, two-tailed test.
**$p \le .05$, two-tailed test.
***$p \le .01$, two-tailed test.

thousand. For cases involving large amounts the increase was from .04 per thousand in 1960 to slightly more that .16 per thousand in 1980. Among cases involving small amounts the increase was from slightly more than .10 per thousand in 1960 to approximately .45 per thousand in 1980. Thus, an increase did occur in the litigation rate during the study period, and it was a general increase occurring across a range of case types.

While Figure 1 indicates that caseloads have increased in Spain over the study period, it provides no indication of how that increase is linked to social development, political transition, or changes in the availability of courts.[9] The influence of these factors on the total litigation rate is examined in Table 1 for the country as a whole. Equation 1 includes the number of courts as a control variable, a dummy variable for each of the three periods of political transition, and the national gross domestic product lagged one year. The one-year lag allows time for changes in the economy to filter through the social system into the legal process.[10] Consistent with the expectations of the social development argument, GDP has a statistically significant positive effect on the litigation rate. An increase of one trillion pesetas in GDP is predicted to yield an increase of approximately one (.9) additional civil case per thousand population. Over the period of study, the GDP rose from roughly 1 trillion to 3½ trillion 1970 pesetas. The pattern of results for the political transition variables also suggests a positive relationship between democratization and the use of the courts.[11] Even with the effects of the supply of courts and the level of social development taken into account, the litigation rate is higher in each period examined. The litigation rate in the post-1977 period is slightly less than one per thousand (.88) higher than in the 1960–68 comparison period. This pattern of increases in litigation with the transition from authoritarianism to democracy is consistent with generalized participation hypothesis but is contrary to the expectations of the alternative participation hypothesis. The latter anticipated a decreased use of courts as orthodox means of political participation became available.

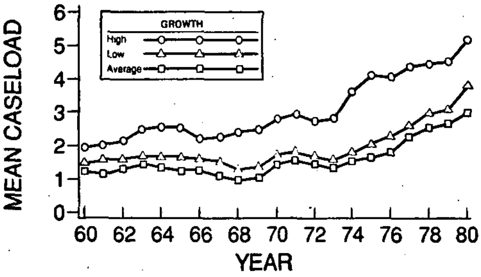
Judged either in terms of mean standard error (MSE) or explained variance, the model provides a good fit to the data. However, given that these data are time series, correlation among the disturbances may result in an underestimation of the error component. Thus, the fit may be over estimated, and, as a result, the tests of significance invalid. A Durbin-Watson coefficient is computed to test for the presence of autocorrelated errors. The coefficient falls within the inconclusive range, providing no basis to accept or reject the null hypothesis of no autocorrelation. As a precaution, first and higher-order autoregressive processes were estimated (analysis not shown). None of the estimates of autocorrelation were statistically significant, suggesting that the ordinary least squares results reported are not distorted by autocorrelation.

An examination of the residuals indicates that the observed litigation rate for 1980 was more than two standard deviations above the value estimated by equation 1. This suggests that the results of the analysis may be distorted by the inclusion of 1980. Accordingly, the model is reestimated with 1980 deleted. The results of this analysis appear as equation 2 in Table 1. The pattern of results is precisely the same as in equation 1, but the overall fit of the model is improved with the MSE declining by roughly one-third. The effects of the political transition variables are more clearly delineated, with the effects of all three obtaining standard levels of statistical significance despite the decrease in degrees of freedom. With 1980 removed, the Durbin-Watson coefficient falls within the range of accepting the null hypothesis of no autocorrelation. While 1980 appears to be an outlier in this analysis, its higher litigation rate may signal the effect of a new period in the transition to democracy. However, this possibility can only be assessed as data become available on litigation rates in more recent years.

The sensitivity of the results to the operationalization of social development was examined by reestimating the model, substituting the percentage of GDP attributable to service and industry. This measure was lagged one year. The results for all years appear in equation 3, and the results for all years except 1980 in equation 4. While the coefficients differ in magnitude, the pattern is comparable to that in equations 1 and 2. Relative growth in the modern segment of the economy is positively associated with increases in the civil litigation rate. An increase of 10% in the GDP attributable to the service and industrial sectors is estimated to increase the civil litigation rate by approximately 1.6 cases per thousand. With 1980 included or excluded, each succeeding time period yields a statistically significant increment to the rate of civil litigation. Thus, regardless of whether social development is operationalized in terms of gross domestic product or the percentage of GDP attributable to the modern sectors of the economy, the results are consistent.

The pattern of increases in Figure 1 suggest the possibility that the effect of social development on litigation may be conditioned by the political environment. The authoritarian political climate in the 1960–68 period may have suppressed the positive effects of economic growth in litigation. The transition to democracy in the 1969–80 period would thus not only directly stimulate increases in litigation but would also allow the effects of economic growth to operate. To examine this possibility three interaction terms were computed by multiplying the dummy variables for the transitional periods by lagged GDP. These terms were then added to the model and equations 1 and 2 were reestimated (analysis not shown).[12] The inclusion of these interaction terms allows the effect of GDP to vary in each period examined. None of these interaction terms approached statistical significance, suggesting that the effect of GDP on the litigation rate did not vary significantly across the periods of political transition.

Figure 2. Contested Civil Cases per 1,000 Population by Year and Economic Growth of Provinces



To this point the analysis has concentrated on the national litigation rate. It may be the case that the results simply reflect changes in a small subset of provinces. If Spanish litigation rates are responding to the transition of the political context from authoritarianism to democracy, the pattern of increases revealed in Figure 1 for the nation should be reflected in the litigation rates of the provinces regardless of their level of economic growth. For each of the 50 provinces the percentage growth in GDP between 1960 and 1980 was calculated. The provinces were then divided into three groups; a low-growth group consisting of the 6 provinces whose percentage growth in GDP was greater than one standard deviation below the mean for all the provinces; an average-growth group consisting of 36 provinces whose percentage growth fell within plus or minus one standard deviation of the mean for all provinces; and a high-growth group consisting of the 8 provinces whose percentage growth in GDP was greater than one standard deviation above the mean for all provinces. For the provinces in each group the mean litigation rate, the lagged mean GDP, and the total number of courts were calculated for each year of the study period.

The litigation rates between 1960 and 1980 for each of these groups of provinces are arrayed in Figure 2. While the intercepts differ, as would be expected given that the groups vary in their average level of GDP, the pattern of increases across the three groups is roughly comparable. Particularly for the low- and average-growth provinces, the increase in litigation becomes most apparent in the post-1969 period. In the high-growth provinces consistent increases in litigation were occurring between 1960 and 1968, but the increases were accentuated between 1969 and 1980. Thus, regardless of whether provinces experienced low or high growth in GDP, litigation rates increased substantially during the transition to democracy.

In equation 1 of Table 2 the effects of social development, political transition and the supply of courts are examined within each subset of provinces. The most striking finding is the consistent pattern of results. Despite the attenuation in its variation resulting from grouping, the effect of GDP is positive and statistically significant within each group. Even with GDP and the supply of courts taken into account, the litigation rate increases with each transitional period. In all three groups of provinces, the litigation rate is significantly higher in the post-1977 democratic period than in the pre-1968 authoritarian period.

An examination of the residuals from equation 1 indicates that 1980 is an outlier falling more than two standard deviations from its predicted value within the average- and high-growth provinces. Taking these findings into account, the model is reestimated without 1980. The results of this analysis appear as equation 2 of Table 2. With this correction the overall fit of the model improves in the average- and high-growth provinces, but the pattern of results are consistent with those in equation 1. The Durbin-Watson statistic for equation 2 falls within the acceptable range for each group of provinces. Thus, the ordinary least squares results reported do not appear to be distorted by autocorrelation.

Conclusions

The present study has examined the effects of the unprecedented social development and the dramatic political transition in Spain since 1960 on the use of the legal system for the resolution of private disputes. It is widely assumed that as societies modernize, the dependence on the courts to resolve disputes will increase. However, previous studies have failed to find a consistent relationship between various indicators of modernization and litigation rates. The present study suggests that this may result from the failure to account for the inherently nonlinear linkage between social development and litigation. Once a society is sufficiently developed to erode traditional norms suppressing litigation and to undermine the basis from informal resolutions, further

Table 2. Regression of Spanish Provincial Litigation Rates, 1960–80, by Provinces

Variables	Equation 1 (All Years)	Equation 2 (1980 Omitted)
Low-growth provinces		
GDP	$.06 \times 10^{-3}$**	$.05 \times 10^{-3}$**
1969–74	.26	.26
1975–76	.25	.32
1977–80	.90***	.93***
Number of courts	.08***	.07***
Intercept	−5.16***	−4.24**
R^2	.95	.94
Mean standard error	.16	.14
Durbin-Watson statistic	1.78	2.01
Average-growth provinces		
GDP	$.03 \times 10^{-3}$*	$.02 \times 10^{-3}$*
1969–74	.13	.14
1975–76	.22	.36
1977–80	.92*	.93***
Number of courts	.01**	.01**
Intercept	−2.91	−1.74
R^2	.90	.93
Mean standard error	.24	.15
Durbin-Watson statistic	1.43	1.86
High-growth provinces		
GDP	$.02 \times 10^{-3}$*	$.01 \times 10^{-3}$*
1969–74	.07	.04
1975–76	.80	.85**
1977–80	1.24**	1.33***
Number of courts	−.01	−.02
Intercept	2.13	4.09*
R^2	.94	.95
Mean standard error	.28	.22
Durbin-Watson statistic	1.57	1.96

*$p \leq .10$, two-tailed test.
**$p \leq .05$, two-tailed test.
***$p \leq .01$, two-tailed test.

development may not effect the litigation rate or may effect it only weakly. Thus, unless a study focuses on the initial period of transition, no relationship will appear to exist. The relatively underdeveloped status of Spain prior to 1960 and its rapid economic growth and attendant social change in the period following 1960, has provided an opportunity to examine the effect of social development within such a period of transition. The results clearly support a relationship between social development and increased use of the courts. Whether operationalized as gross domestic product or as the percentage of the gross domestic product attributable to service and industry, increases in social development were found to be positively linked to increases in the litigation rate in Spain. These results are in marked contrast to those of Toharia (1974), who examined changes in the litigation rates in Spain between 1960 and 1969. While Toharia's conclusions were reasonable based on the years examined, the trend between 1960 and 1969 appears quite different from the perspective of an additional decade of evidence.[13]

While it has been argued powerfully that litigation is a form of political participation (e.g., Zemans 1983), the evidence to support this position has been meager at best. The Spanish case has provided an opportunity to assess the linkage between litigation and the political context in the presence of a regime change from authoritarianism to democracy. The results of this analysis clearly support a linkage between the political context and litigation. As the authoritarian political environment—which denied mass participation in the political system and suppressed public conflict—deteriorated and was replaced by a democratic environment that encouraged participation and accepted public conflict as legitimate, litigation rates increased in Spain.[14] This is consistent with the generalized participation hypothesis but contrary to the alternative participation hypotheses. This pattern of increase persisted despite controls for social development and for the supply of courts and was evidenced both at the national level and among provinces at various levels of economic growth. The strength of the finding is also suggested by the fact that it occurs despite the use of surrogates to measure the political transition and the fact that the final stages of the transition, when the effects of democratization might be expected to be greatest, were not included in the study period.

While the case of Spain indicates that the emergence of a democratic political context contributes to the use of the courts, it also places in sharp relief the issue of the contribution of litigation to the democratic nature of a political system. Under Franco the civil courts were relatively independent institutions. Their independence arose from the fact that their jurisdiction was wholly of a private nature, lacking a public law component. Unlike the U.S. courts, the Spanish courts could not be employed to challenge state actions or authority. In essence the Spanish courts were independent because they were politically impotent (Toharia 1974).[15] Was Spain a more democratic society because the courts were independent and could be seized directly by private citizens to pursue their private allocations of values? Or was judicial independence and the accessibility of the courts part of the Machiavellian web of authoritarianism, which saw an independent and accessible legal system as a powerful yet nonthreatening symbol? While access to the courts for civil suits may be a "quintessentially" democratic form of participation, the Spanish case makes clear that absent a democratic political system, it is not a very potent one. Moreover, without the encouraging norms, structures, and processes of a democratic political context, the results of the present study suggest that the exercise of litigation as a form of participation may not obtain its full potential.

Notes

An earlier version of this article was presented at the 1987 annual meeting of the American Political Science Association.

1. The literature developing this hypothesis speaks largely in the idiom of political culture. Rather than enter the mire of the debate on political culture, we prefer to use the terms *context* or *environment*.

2. Given such slow and limited development and the intervention of a major depression, political turmoil, and a civil war, it is easy to see how Toharia, using gross aggregate data, might have difficulty discerning a relationship between development and litigation rates.

3. In statistical reports a distinction is made between "contested" and "voluntary" cases. The first refers to cases in which two parties are opposed. The second to cases in which the court acts without an opposing party, such as in the uncontested settling of an estate. We have focused on the contested cases because they provide a more appropriate test of the theories under examination. However, the analysis was repeated on the total case load (contested + voluntary) and the pattern of results was the same as reported here for contested cases only.

4. The constitution created a Constitutional Court outside of the structure of the general courts and limited the jurisdiction of martial courts to strictly military concerns. More recently the Ley Organica (Organic Law) of 1 July 1985 has altered the structure of the general courts, but the changes embodied in the general law do not alter the jurisdiction or function of the courts of first instance and occur after the period of study. Some minor shifts in jurisdiction did occur around 1968, but our analysis of these changes and our interviews with knowledgeable Spanish authorities indicate that no structural or jurisdictional changes can explain the changes found in the litigation rates. Interviews with Spanish authorities also do not indicate any significant changes in access to the courts (e.g., threshold costs), in the litigation role of the legal profession, or in the availability of alternative dispute resolution mechanisms during the study period.

5. Case filings were not available by substantive categories. This information was only available for case terminations. Except in the last few years of the period examined, the gap between filings and terminations was very low. Hence, we do not believe that this difference has affected our conclusions.

Prior to 1968 the division between large and small cases was 500 thousand pesetas. After 1968 the division was a million pesetas.

6. Toharia (1974) also examined a category of cases dealing with estates. This category of cases is not treated consistently through the study period and hence is not included in the analysis. In fact, other than negotiable instruments and small and large monetary amounts, few of the categories under which case terminations are reported have remained constant over the study period. This limits detailed analysis of Spanish litigation rates by case categories.

7. Indeed, there was a failed attempt at a military coup in February 1981.

8. Since courts of first instance employ a single judge, the number of courts is equivalent to the number of judges.

9. Spain in the 1970s experienced increases in unemployment and business failures. It might be argued that the growth in case loads shown in Figure 1 simply reflects these conditions rather than the effects of social development, democratization, and court availability. This, however, does not appear to be the case. First, while case loads in Spain rise irregularly after 1968, unemployment does not show any pattern of increase until after 1973 and does not rise above 5% until 1976. Second, cases involving bankruptcies and unpaid negotiable instruments did increase disproportionately between 1967 and 1968. However, while the numbers of such cases continued to increase after 1968, the number of other types of cases also rose. As a result, cases involving bankruptcy and unpaid negotiable instruments constitute a relatively constant percentage of all contested cases in the years from 1968 to 1980. Hence, the growth in case loads in the post-1968 period cannot be attributed simply to an increase in cases involving bankruptcies and unpaid negotiable instruments.

On more theoretical grounds Stookey (1986) convincingly argues with support from the U.S. case that economic recessions do not produce the pattern of long-term growth in litigation evident in Figure 1.

10. Lagging GDP increases the estimates of the effect for social development slightly and reduces the estimates for political transition. The overall pattern of results, however, is unchanged. A two-year lag was also examined and the result were virtually identical to those produced by a one-year lag.

11. The intercept in this and the following equation can be interpreted as the litigation rate in the suppressed years, 1960–68, adjusted for the effects of GDP and number of courts. If GDP and courts were both equal to zero, the estimated litigation rate would be 5.12 for these years. This is an impossible result, but the assumptions are also unrealistic. In 1960 GDP in current 1970 pesetas was 1,162,700, and there were 579 courts of first instance. Adjusting the intercept for these values yields an estimated litigation rate of 1.71, close to the observed value of 1.86 in 1960.

12. The model estimated was

$$Y = a + b_1\mathrm{LGDP} + b_2D1 + b_3D2 + b_4D3 + b_5\mathrm{courts} + b_6(D1^*\mathrm{LGDP}) + b_7(D2^*\mathrm{LGDP}) + b_8(D3^*\mathrm{LGDP}) + e,$$

where Y = litigation rate, D1–D3 = dummy variables for various time periods, LGDP = lagged GDP, courts = number of courts, b_1–b_8 = regression coefficients, a = intercept, and e = error.

13. Toharia (1974) also failed to take into account the effects of changes in the supply of the courts. In the latter portion of the period he examined, the number of courts actually decreased, which would tend to offset the positive effects of economic growth. In fact, GDP has a positive significant effect on the litigation rate between 1960 and 1969, once the effects of the supply of courts is taken into account.

14. This is not to say that interests that were precluded from expression in the political arena during the Franco period did not use the courts to articulate their positions. However, the number of such cases was probably small relative to the total volume of civil cases. Hence, their removal from the courts to the political arena as the latter democratized would not yield a noticeable decline in litigation rates.

15. Under the new constitution the ordinary courts continue to have no jurisdiction over public law cases. However, through the use of writs of amparo and the power of constitutional review, the newly created Constitutional Court does consider such issues.

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ELECTORAL LAWS, PARTY SYSTEMS, AND ELITES: THE CASE OF SPAIN

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Using aggregate, survey, and in-depth elite interview data from Spain in the 1970s and 1980s, I demonstrate that the "mechanical" effect of the Spanish electoral law is as strong as that of many single-member constituency systems. But the "distal" effect of the electoral law on the party system is shown to be complex and multifaceted, not direct and deterministic. The perceptions, calculations, strategies, and behavior of party elites play a crucial intervening role between the electoral law and the overall shape of the party system.

For over a century scholars have analyzed and speculated about the impact of electoral laws on the structure of party systems. Since the publication of Duverger's *Political Parties* in 1951, the "mechanical" or "proximal" effects of electoral systems have been demonstrated several times (Duverger 1954, 226; Rae 1971, 67). Electoral laws have been shown to vary significantly and predictably in the extent to which they allocate seats in legislative bodies in relation to each party's respective share of the popular vote. The "psychological" effect posited by Duverger (1954, 226; also see Downs 1957, 48) has also received considerable, but not uniform empirical confirmation. Several studies (Bensel and Sanders 1979; Cain 1978; Fisher 1973; Spafford 1972) have turned up evidence of "sophisticated" or "strategic" voting.

Up to now, however, the "distal effects" of electoral laws on the structure of party systems (Rae 1971, 68) have not been examined with much precision. Explorations of the long-term impact of electoral laws on the basic structure of party systems have consisted largely of speculative inferences: the correlations between proportional representation systems and multipartism, on the one hand, and between plurality electoral laws and two-party systems, on the other, are simply regarded as reflections of this long-term, or distal, effect. Prominent exceptions to the general patterns (e.g., Canada and Austria), however, have fueled debate over the adequacy of causal assertions regarding this relationship. Riker (1986), for example, expressed concern over whether Duverger's propositions should be regarded as "hypotheses" or "laws" and whether they are "probabilistic" or "deterministic." Sartori (1986) entered into debate over these objections and added several of his own concerning such questions as whether the influence of electoral laws on party systems should be regarded as "effects" or "side effects," and whether they "produce" or merely "maintain" certain kinds of party systems.

Given the complex nature of the phenomena under investigation and the imperfect empirical association between electoral laws and party systems, I doubt whether understanding of these distal effects can be advanced much further by conducting this debate at such high levels of abstraction. Instead, I concur with Rose, who argued that a significant advance in this line of inquiry can only be achieved by broadening the scope of analysis: "To understand the working of electoral systems in practice, we must consider both generic properties of the system and specific national contexts. . . . The workings of an electoral system cannot be understood simply in terms of definitional and abstract properties; they must be evaluated by the analysis of an electoral-system-in-a-political system" (1984, 78). This suggestion is consistent with an earlier assertion by Rae, who warned that inferences about the distal effects of electoral laws on party systems "must be made with the greatest caution, since observed associations may reflect the influence of intervening or underlying factors in the political system which are independent of the electoral law" (1971, 68).

This analysis represents an effort to explore one crucial set of such intervening factors—the perceptions, calculations, strategies, and behavior of party elites, in interaction with the incentive structure created by the electoral law itself. It begins with an examination of the mechanical and psychological effects of Spain's electoral law on the partisan allocation of seats. This portion of the analysis is based on aggregate data dealing with Spain in the 1970s and 1980s, as well as two large post-election surveys conducted in 1979 and 1982.[1] One aspect of this analysis involves simulations (designed and carried out by Giacomo Sani) of varying combinations of parties as electoral slates, which serve as a basis for speculation about the likely outcomes of differing coalition, merger, or schism possibilities.[2] The centerpiece of this study—an explanation of reasons why party elites may behave in a manner inconsistent with the incentive structures inherent in the electoral law—is based on extensive in-depth interviews with Spanish political leaders at various stages during the transition to, and consolidation of the current democratic regime.[3]

Spain presents an excellent opportunity to measure the broader impact of an electoral law on a new and evolving party system. The electoral law adopted in 1977 is a substantially restricted form of proportional representation whose "strength," or departure from pure proportionality in allocating parliamentary seats, is comparable to that of single-member district systems. As will be demonstrated below, there is considerable evidence of both the mechanical and psychological effects of this electoral device. As one might expect, given the introduction of a strong electoral system into a new and highly fragmented party system, the electoral law exerted considerable influence in the first democratic election in simplifying the structure of partisan competition. The electoral law overrepresented the two largest nationwide parties and substantially underrepresented small parties with geographically dispersed bases of support, denying the vast majority of them parliamentary representation. In combination with party-financing laws (which reinforced the biases inherent in the electoral law),[4] this drove many parties out of existence in the immediate aftermath of the 1977 election.

The two-dominant-party system that took shape at that time (and was reaffirmed in the 1979 contest) appeared to "make sense" insofar as it was a logical outgrowth of both the electoral law and the distribution of political values and preferences of the mass public (see Gunther, Sani, and Shabad 1986.) But the alignment of partisan forces that emerged from the first two elections was unstable despite its compatibility with these institutional and social-structural underpinnings. Instead, the 1982 turnover in parliamentary seats drastically restructured the party system and led directly to the extinction of Spain's once-governing centrist party. The representational biases inherent in the electoral law played an important role in producing this outcome and have served as an obstacle to the emergence of a new centrist party as a successor to the now defunct Unión de Centro Democrático (UCD).[5] Thus, the Spanish electoral law has continued to exert a powerful impact on the structure of partisan competition.

The case of Spain is also of considerable interest because it illustrates the extent to which the shape of a party system may be substantially influenced by the independent actions of party elites. Despite widespread awareness of the influence of the electoral law over prospects for receiving parliamentary representation, many of them developed strategies and engaged in behavior that in light of the incentive structure inherent in the electoral law, were irrational if not suicidal. In short, this set of "intervening" factors has affected the structure of partisan competition in a manner one could not have predicted on the basis of the logic of the electoral law alone.

A detailed analysis of the impact of the electoral law on the Spanish party system suggests several modifications in the conventional understanding of the distal impact of electoral systems. Briefly stated, they imply that these distal effects would be direct, uniform, and predictable only if certain assumptions (involving such factors as the primary motivations of party elites and certainly about levels of electoral support for each party) are strictly adhered to. The history of parties of the center and right after the death of Franco clearly reveals that at several crucial junctures these conditions were not met.

The Spanish Electoral Law: Its Origins and Consequences

The electoral law enacted by royal decree on 18 March 1977 was the product of a complex set of deliberations, involving discussions between Prime Minister Adolfo Suárez and representatives of various opposition forces. These decisions involved trade-offs and compromises among a wide variety of both partisan and broader societal concerns. The choice of an electoral system in advance of Spain's first democratic election was of particularly great significance from the standpoint of the legitimacy and viability of the new regime itself. A difficult balance had to be struck between the need to create a party system conducive to stable government and the need to represent the interests of significant political and social groups. On the one hand, excessive fragmentation of the new party system had to be reduced in order to facilitate the formation of governments with the ability to enact legislation and carry out those programs over reasonable periods of time. This was a particularly salient concern in early 1977, when over 100 political parties came into existence (78 of which fielded candidates and received votes in the 15 June election). The experience of the Second Republic suggested to many informed observers that excessive parliamentary fractionalization would be detrimental to the prospects for governability.[6] On the other hand, the exclusion of significant political forces from a constituent parliament could threaten the legitimacy of the new regime. Groups excluded from deliberations over establishment of the basic institutions and procedures of a new democracy may, simple because of their exclusion, reject whatever constitution may be produced. Conversely, groups directly involved in the constituent process are more likely to conclude that they have a stake in preserving the new regime that they have helped to create (see Gunther 1985).

These choices were also complicated by calculations of partisan self-interest. It is not surprising to find that political groups favored adoption of different electoral systems in accord with their estimations of the size and geographical distribution of their respective blocs of supporters. Thus, one party whose leaders believed that it would receive pluralities of popular votes in most parts of the country, the right-wing Alianza Popular (AP), favored adoption of a single-member constituency system, expecting that this system would magnify that popular vote advantage into a larger plurality or majority of seats in parliament. Conversely, smaller parties with geographically dispersed bases of support, such as the Partido Comunista de España (PCE), favored proportional representation formulas. And purely regional parties, such as the Basque and Catalan nationalist parties (Partido Nacionalista Vasco [PNV], Convergència Democrática de Catalunya [CDC], and Esquerra Republicana de Catalunya [ERC]) were greatly concerned with how the boundaries of electoral districts were drawn. The selection of an electoral system was (and was widely regarded as) crucial insofar as it directly affected each party's prospects for staking out a dominant, or at least visible, position on the political scene.

The electoral law enacted by the Suárez government represented a compromise among the conflicting demands of the various parties. In accord with the government's initial preferences and the demands of the Communists and Socialists (both Partido Socialista Obrero Español [PSOE] and Tierno's Partido Socialista Popular [PSP]), proportional representation principles were endorsed in the 1976 Political Reform Law and the 1977 electoral law. However, in order to secure Cortes approval of the reform law (no small task, since Franco's final Cortes was being asked to commit institutional suicide and begin to dismantle the old regime), Prime Minister Suárez depended on the votes of many *procuradores* affiliated with the newly created Alianza Popular. This enabled the latter to bargain successfully for inclusion in the electoral law of "correctives," designed to reduce party-system fragmentation and, as a by-product, to reduce the proportionality of representation in the future Congress of Deputies. Thus, the reform law stipulated that the Congress would have 350 members, that a minimum threshold of votes would be required to obtain parliamentary representation, that electoral districts would normally coincide with the 50 provinces, and that each province would be entitled to a minimum number of representatives. The 1977 electoral law resolved several issues left open by the reform law of the previous year, most importantly by determining that the D'Hondt "highest average" method of seat allocation would be used and that, in practice, the smallest provinces would receive a minimum representation of three seats.

Mechanical Effects

There are two sources of representational bias in this electoral system. The first derives from the law's efforts to achieve "greater territorial equilibrium in representation" (Ministerio del Interior, Secretaría General Técnica 1979, 27) by overrepresenting small provinces in the Congress. This meant, for example, that in 1977 rural and sparsely inhabited Soria would send one deputy to the Cortes for every 34,636 of its inhabitants, while the district of Madrid would receive one deputy for every 139,569 of its citizens.

The second source of representational bias is the joint product of the existence of many small districts and use of the D'Hondt system of seat allocation. Rae has argued that "the D'Hondt formula will . . . penalize small parties in the extreme unless the district magnitude is very large or the party system is highly fractionalized," that is, where "competitive strength is . . . divided among many [parties]" (Rae 1971, 33, 53). I have demonstrated elsewhere (Sani and Gunther 1986) that fractionalization is reasonably high

Table 1. Popular Votes and Seat Allocations in Two Districts, 1979

Parties	Votes	Votes (%)	Seats	Seats (%)	Votes per Seat
Madrid					
PSOE	769,328	34.9	12	37.5	64,111
UCD	764,830	34.7	12	37.5	63,736
PCE	310,496	14.1	4	12.5	77,624
AP	198,345	9.0	3	9.4	66,115
UN	110,730	5.0	1	3.1	110,730
ORT	48,345	2.2	0	0	—
PTE	25,832	1.2	0	0	—
Zamora					
UCD	61,025	51.0	3	75	20,342
PSOE	27,143	22.7	1	25	27,143
AP	19,603	16.4	0	0	—
PCE	3,506	2.9	0	0	—

in only about one-quarter to one-third of Spain's provinces, mainly in areas where regional parties are strong; hence this mitigating factor is absent from most districts. The other mitigating factor applies only to a handful of provinces, since the great majority of districts are small: 7 provinces are represented by 3 deputies, 8 provinces by 4 representatives, and 13 districts send only 5 deputies to the Cortes. Rae (1971, 116–17) suggests that proportional representation systems will be reasonably proportional in their allocation of seats only in districts represented by about 7 or more deputies: only 19 of Spain's 50 provinces fall into this category, and only 6 districts elect 10 or more members of the Spanish Congress. Significant representational biases affect the allocation of seats in other districts; and the smaller the district, the more the distortions approximate those inherent in single-member constituency systems.

The net impact of the D'Hondt system, small district size, and the overrepresentation of sparsely populated rural constituencies can best be seen by comparing two provinces: Madrid, represented by 32 deputies; and Zamora, which elects 4 representatives. As can be seen in Table 1, the large size of the Madrid district results in a reasonable approximation of proportionality in the allocation of seats, but the underrepresentation of the district as a whole means that the ratio of elected deputies to popular votes is extraordinarily high. By way of contrast, the UCD's advantage in popular votes in Zamora was magnified (as a result of small district size) into a huge representational majority. In addition, it takes far fewer popular votes to elect a deputy in Zamora than it does in Madrid.

These representational biases were of considerable political significance for the overall shape of the party system, making it possible for the UCD to form single-party minority governments in 1977 and 1979 even though it received little over one-third of the popular votes cast in those elections. This mechanical impact of the electoral system can be appreciated by examining the 1979 election results in the Castilla-Leon region, of which the province of Zamora was a part. The small average district size in that region (4 of its provinces elected only 3 deputies, 3 elected 4 representatives, and the remaining 2 elected 5 and 6 deputies, respectively) magnified the UCD's plurality of the popular vote into a huge majority in terms of parliamentary representation. The UCD

Table 2. Votes and Seats in Congress of Deputies, 1977–86 (%)

Parties	1977 Votes	1977 Seats	1979 Votes	1979 Seats	1982 Votes	1982 Seats	1986 Votes	1986 Seats
Nationwide								
PSOE	28.9	33.7	30.5	34.6	48.4	57.7	44.4	52.6
UCD	34.0	47.1	35.1	48.0	6.9	3.1	—	—
AP	8.0	4.6	6.1	2.6	26.4	30.6	26.1	30.0
PCE	9.2	5.7	10.8	6.6	3.9	1.1	4.7	2.0
PSP[a]	4.4	1.7	—	—	—	—	—	—
CDS	—	—	—	—	2.9	.6	9.2	5.4
Others[b]	6.5	0	6.4	.3	2.5	0	3.7	0
Regional								
PDC/CiU	2.8	3.1	2.7	2.3	3.7	3.4	5.1	5.1
PNV	1.7	2.3	1.7	2.0	1.9	2.3	1.5	1.7
EE	.3	.3	.4	.3	.5	.3	.5	.6
HB	—	—	1.0	.9	1.0	.6	1.2	1.4
Others[b]	3.9	1.4	5.0	2.6	1.9	.3	3.6	1.1

[a] It should be noted that three of the PSP's six seats in the Cortes were the results of entering into regional coalitions.

[b] Nationwide: UN; regional: ERC, Partido Socialista de Andalucía, Unió del Centre i la Democracia Cristiana de Catalunya, Candidatura Aragonesa Independiente de Centro, Candidatura Independente de Centro (Castellón), Unión del Pueblo Navarro, Partido Aragonés Regionalista, Agrupaciones Independientes Canarias.

received 49.9% of the votes cast throughout the region but elected 70.5% of its deputies to the Cortes—31, or 9 more than it would have been entitled to if a single regionwide district had existed, instead of 9 separate provincial districts. Overrepresentation of the UCD in regions like Castilla-Leon created a "manufactured" near-majority for the party that in combination with the constitution's "constructive vote of no-confidence," made it possible for the party to govern alone from 1977 to 1982.

Even though the representational biases seen in the province of Zamora are offset somewhat by the existence of large provinces like Madrid and Barcelona (whose 32 and 33 seats produce a reasonably proportional distribution of seats), there are enough provinces like Zamora to produce a strong mechanical effect overall. In general, the two largest parties are overrepresented in the Congress of Deputies; smaller parties with geographically dispersed bases of support are underrepresented (and the smaller the party, the more devastating its underrepresentation); while parties with geographically concentrated bases of support (such as the Basque and Catalan nationalist parties) receive more-or-less proportional representation in the Congress of Deputies. These representational biases may be seen in Table 2, which presents electoral data from all four of Spain's democratic elections to this point.

What is most striking is that these representational biases are stronger than those found in any system currently in use in Western industrialized societies and are, in fact, as strong as those found in some single-member constituency systems. Table 3 places Spain in rank order with other Western democracies in terms of its score on Rose's Index of Proportionality. As can be seen, the Spanish electoral system is strikingly unproportional.

These data also give some evidence of

the distal effect on the Spanish party system. The steady increase in Spain's proportionality score with each successive election is a reflection of the fact that the electoral party system is progressively coming into line with its parliamentary party system.[7] In 1977, for example, parties once thought to be viable competitors for positions of governmental responsibility (such as Enrique Tierno Galván's PSP and the Christian Democratic "Equipo") were drastically underrepresented or denied parliamentary representation outright, driven into bankruptcy, and forced out of existence. But Spain's continuing low scores also suggest that there has been considerable "lag" in this process—that some parties are reluctant to conform to the dictates of the electoral law. This finding, as well as the clearly "irrational" behavior exhibited in 1982 by important party leaders, will be examined later.

Sophisticated Voting

Apart from the mechanical effects observed above, Spain's electoral law has given rise to a considerable amount of "sophisticated" or "strategic" voting, indicating that the psychological effect hypothesized by Duverger is very much at work. Some previous research (e.g., Grumm 1958; Shively 1970) found little evidence in support of Duverger's claim that under certain kinds of electoral law, voters will cast ballots for parties other than their preferred first choice rather than risk "wasting" their votes on a party with no real prospects of victory. One treatise (Meehl 1977) even contended that since no individual voter can seriously expect to determine the outcome of an election, the concept of the "wasted vote" should not even exist. Consistent and substantial evidence from our survey data, however, clearly reveals that the Spanish voter is very "sophisticated" and is quite concerned about casting a *voto útil* (the opposite of a wasted vote).

A proper test of "psychological" influences on voting behavior obviously requires individual-level data. Fortunately, the 1979 and 1982 postelection survey data, in combination with information concerning the differing provincial electoral law contexts within which the respondents made their voting decisions,

Table 3. The Proportionality of Electoral Systems

Electoral Systems	Index of Proportionality[a]
Proportional representation systems	
Austria	99
Germany	98
Denmark	97
Ireland	97
Netherlands	97
Sweden	96
Iceland	96
Switzerland	96
Finland	95
Italy	95
Israel	94
Belgium	94
Portugal	93
Norway	91
Luxembourg	90
Greece	88
Average	95
Spain (1986 election)	87
Spain (1982 election)	86
Spain (1979 election)	83
Spain (1977 election)	81
Non-proportional representation systems	
United States (House of Representatives, 1980)	94
Japan	91
Canada	88
Australia	87
Britain	85
New Zealand	80
France	79
Average	86

Source (except for Spain): Rose 1984, 75.

[a] The sum of the absolute values of the differences between each party's share of seats and its share of votes, divided by two and subtracted from 100.

make it possible to test for the presence of strategic or sophisticated voting. In examining such contextual effects, however, it is first necessary to control for individual-level determinants of the vote. Unless this is done, there is too great a risk that a constituency size variable might actually be standing in for social-structural, economic, or cultural factors that affect voting decisions. One should not jump to the conclusion, for example, that an ecological correlation between small constituency size and low levels of support for the Communist party is a product of a desire by PCE sympathizers not to waste their votes; instead, it could reflect the fact that individuals residing in such provinces tend to be conservative farmers who never had any intention of casting ballots for the Communist party. In order to control for these individual-level factors, the following analysis examines the voting behavior of only those individuals who were strongly predisposed to support a particular party. Extensive multivariate analysis of the 1979 survey data found that the strongest predictor of the vote for a party was a respondent's evaluation of that party using an 11-point "feeling thermometer," ranging from 0 to 10 (see Gunther 1986a, n.d. [a]). In testing the wasted vote—or in this instance, *voto útil*—hypothesis, only individuals giving a particular party a 9 or 10 on the feeling thermometer in 1979 were examined.

In accord with the biases inherent in the electoral law, respondents who evaluated the two largest nationwide parties in these highly favorable terms strongly tended to cast ballots for those parties: 76% of those giving the UCD a 9 or 10 on the feeling thermometer voted for that party; and 62% of respondents similarly predisposed toward the PSOE cast ballots for the Socialist party. Among those favorably disposed toward the third- and fourth-place parties nationally, however, levels of electoral support were substantially lower: 45% of those rating the PCE in the top two feeling thermometer categories actually voted Communist; and only 31% of those giving the AP a 9 or 10 cast ballots for that conservative party.

This prima facie case for the presence of sophisticated voting is strongly corroborated when the voting behavior of these respondents is broken down by province in accord with the number of deputies sent to the Cortes from each district. Respondents with highly favorable attitudes toward the third- and fourth-place parties in large provinces were about twice as likely to vote for them as sympathizers of those same parties in small provinces. What is most striking is the apparent ability of Spanish voters to determine the threshold levels separating provinces in which their preferred party had a fair chance of receiving parliamentary representation from those in which their votes would have been "wasted." Applying the D'Hondt "highest average" calculations to a hypothetical constituency whose popular votes exactly mirror the levels of electoral support received by parties nationwide (see Table 2), for example, the PCE would have received at least one seat in districts electing five or more deputies, and would have been denied parliamentary representation in smaller districts. This threshold level is reflected in the voting behavior of PCE sympathizers: among respondents rating the Communist party at 9 or 10 on the feeling thermometer in 1979, 52% of those residing in provinces electing five or more deputies actually cast ballots for the PCE ($N = 93$), while in provinces electing only three or 4 deputies ($N = 280$) only 25% did so (Tau $B = .23$, $p < .001$). The AP did not do as well in either the 1977 or 1979 elections, so its representational threshold was higher. Among AP sympathizers residing in provinces electing six or more deputies ($N = 133$), 37% cast ballots for Alianza Popular; but in smaller constituencies ($N = 64$) only 19% did so (Tau $B = .18$, $p < .01$). Conversely, the

UCD—the natural recipient of votes from AP sympathizers not wishing to "waste" their votes—did slightly better in small provinces than it did in large provinces (Tau $B = .07$, $p < .05$).

An examination of the 1982 survey data turns up even stronger evidence of sophisticated voting, and again, the sensitivity of Spanish voters to the structure of the electoral law is quite striking. In this survey, we introduced a different measure of sympathy toward political parties (asking respondents to state if they regarded themselves as "very close," "somewhat close," "rather distant," or "very distant" toward each party), which turned out to be a better predictor of the vote than the feeling thermometer. Using this "proximity" item as a control, the contextual effects of the electoral law are seen to be very strong. Huge majorities of those feeling sympathetic toward one of the two largest parties voted for that party: 84% of those who felt "very close" to the PSOE voted for the Socialist party; and 86% of those similarly disposed towards Alianza Popular voted for AP. Again, smaller parties with geographically dispersed bases of support received much lower levels of electoral support from their respective blocs of sympathizers: 62% of those "very close" to the rapidly collapsing UCD cast ballots for that party; 56% of those highly sympathetic towards the Communist party voted for the PCE; and among those "very close" to the fledgling Centro Democrático y Social (CDS) of Adolfo Suárez, less than one-third actually voted for that party.

A closer inspection of voting behavior by these party sympathizers in conjunction with their geographical distribution strongly suggests that the electoral law did, indeed, effectively discourage individuals in certain contexts from casting ballots for their party of first choice. Since the CDS was widely expected to do poorly in its first electoral contest (one published poll suggested that it would receive only about 2% of the vote),[8] its threshold for receiving parliamentary representation was quite high. This was correctly perceived by voters: 48% of those "very close" to the CDS who resided in provinces electing 12 or more deputies ($N = 34$) cast ballots for Suárez's party; but only 18% of those in smaller provinces ($N = 81$) did so (Tau $B = .31$, $p < .001$).[9] Similarly, significant numbers of Communist party sympathizers residing in small provinces were discouraged from voting for their first choice: just 33% of those "very close" to the PCE who lived in provinces electing five or fewer deputies ($N = 35$) cast ballots for the Communist party, while 61% of those residing in larger provinces ($N = 149$) did so (Tau $B = .22$, $p < .001$). And in the very smallest provinces—electing only 3 or 4 deputies—electoral support for the party among those "very close" to the PCE virtually vanished: only 6% of those respondents ($N = 16$) voted for the PCE, versus 61% of those in larger provinces ($N = 168$, Tau $B = .33$, $p < .001$).

The fact that similar patterns can be identified among supporters of very different kinds of parties—with different socioeconomic and demographic bases of support and very different images and ideologies—lends credence to a contention that large numbers of Spaniards engaged in sophisticated or strategic voting. This interpretation is also consistent with other data collected in a 1982 postelection survey. Individuals who had supported the Communist party in 1979 but abstained or shifted their support to another party in 1982 overwhelmingly gave as either their first or second reasons for abandoning the party their belief that "it had no chance of winning" (see Gunther 1986a, n.d. [a]).

The Strength of the Spanish Electoral Law

What has been the overall impact of the proximal effects of Spain's electoral law on the new democratic regime that came into existence in 1977? In one respect, we can conclude that it has made an unequivocally positive contribution: the "correctives" included in the electoral law have, indeed, achieved their stated objectives of making it easier to form a government and of reducing the fragmentation of the parliamentary party system. It is noteworthy that in none of the four general elections which have taken place since the death of Franco did a party win an outright majority of the popular vote. Nonetheless, for 12 years Spain has been led by single-party governments and has not experienced an unacceptable level of cabinet instability: the Socialists have been supported by "manufactured" majorities in the Congress of Deputies; and the UCD, while never enjoying outright majority support, stayed in power for over five years as a minority government, thanks to the electoral law's magnification of its plurality of the popular vote and to the "constructive vote of no-confidence" built into the constitution.

If a purer form of proportional representation had been institutionalized in Spain (as in Israel, the Netherlands, and Italy), formation of a government supported by a parliamentary majority would have been exceedingly difficult. This would have had particularly serious consequences in the constituent Cortes of 1977–79, and could have greatly restricted Adolfo Suárez's maneuverability in efforts to forge a constitutional consensus (see Gunther 1985, 1987; Gunther and Blough 1981; Gunther, Sani, and Shabad 1986). Indeed, it is even difficult to imagine what kind of coalition could have been formed during that crucial legislative session. If a pure form of proportional representation had been adopted, the broadest possible center-right coalition (including the UCD, AP, the Catalans and the Basque Nationalist party [PNV]) would still have lacked a majority by about 10 seats. And a reincarnation of the Popular Front (including the PSOE, the PCE, Tierno's PSP and the ERC) would have fallen short of a governing majority by over 20 seats. Only a *Grosse Koalition* of the UCD and PSOE (hardly imaginable in those early days of Spanish democracy) could have commanded a secure majority in the Cortes.

The distal effects of the electoral law on the party system, however, have been neither clear-cut nor unidirectional. The mechanical effects of the electoral law, in combination with the widely perceived tendency of Spanish voters to cast the *voto útil* and not waste their votes on smaller parties, should have drastically simplified the electoral party system. Leaders of small parties with geographically dispersed bases of support should have been induced to merge or form electoral coalitions with other parties of comparable ideological bent. Failing to adopt such rational representation-maximizing strategies should have condemned minor parties to extinction. We have seen some evidence of trends in this direction. The steady increase in Spain's proportionality score from 1977 to 1986 indicates that voters have progressively tended to concentrate their support behind parties with some serious chance of winning seats in parliament. And the number of parties fielding candidates and receiving votes in national elections did decline somewhat, from 78 in 1977 to 51 in the 1979 election.

But this process of party system simplification is far from complete. The "winnowing out" process appears to have halted following the first democratic election. In the 1982 and 1986 general elections 59 and 56 parties, respectively, fielded candidates and received votes. The 1986 election, moreover, revealed a reduction in the share of the total vote received by the two largest parties (from 74.7% in 1982 to 70.5%), and the number of parties with representation in the Congress increased from 10 to 12. Moreover, the registration of parties with the Ministry of the Interior has continued unabated: as of 1 January 1987, over 500 political parties were still officially registered (see Tamames 1987, 97–103). Of greatest significance, fragmentation at the center of the political spectrum following the 1982 election has, to this point, impeded the reemergence of a party or coalition with the ability to pose a credible electoral challenge to Felipe González's PSOE. Overall, the persistence of party system fragmentation certainly suggests that the relationship between electoral laws and party system is not direct, automatic and deterministic.

The behavior of party leaders clearly reveals that the evolution of Spain's party system cannot simply be interpreted as a direct response to the incentives inherent in the electoral system. The widely acknowledged representational biases of the system should have served as a powerful deterrent to schisms and party system fragmentation and as an inducement to mergers among small partisan groups. Logically, this should have contributed to stabilization and consolidation of the party system. Instead, the emergence of new parties, schisms within old parties, and stubborn resistance to merge or coalesce with other groups has been a constant theme of partisan politics in Spain over the past decade.

Indeed, the electoral law played a paradoxically *de*stabilizing role in the most dramatic example of this phenomenon—the collapse of the UCD in 1982. Despite the incentive structure inherent in the law, it did not serve as an adequate deterrent to the fragmentation of that once-governing party. Once the process of disintegration had begun, however, it magnified the electoral decline of the UCD and accelerated its slide towards oblivion, contributing over the long term to a fragmentation of the political center. The irrational behavior of party elites in this and related episodes reveals the limitations of the role of electoral laws in determining the outlines of party systems.

Schisms, Coalitions, and Mergers: The Role of Elites

Simplification of the party system would require that party leaders respond to the incentives presented to them by the electoral law by avoiding schisms in existing parties or by forming electoral coalitions with like-minded parties or merging with them outright. This incentive structure, however, is based solely on the assumption that party leaders will engage in this kind of behavior as a means of maximizing their representation in the parliament and that they will place this objective above all others that might influence their behavior. I examine three sets of decisions made by Spanish party leaders during the 1982–86 period that were critical in this regard. In doing so, we shall observe the complexity and multidimensional nature of such choices and explain why the distal effects of electoral laws on party systems are not direct, uniform, and deterministic. This analysis is based upon Giacomo Sani's simulations of various coalition strategies (extrapolating from real data relevant to the two elections under consideration), as well as my extensive in-depth interviews with all of the relevant elites during this period (see Sani and Gunther 1986).

Centrist Unity

The first possible coalition to be examined is one linking the UCD to the new CDS of Adolfo Suárez. For many reasons, this would have been a plausible and potentially profitable combination. Most of those who founded the CDS were, after all, members or leaders of the UCD prior to 29 July 1982. The avoidance of an overt schism is all that would have been required to maintain this "coalition" prior to that time. But even after the formal separation of Suárez from his former party, serious efforts were undertaken to establish an electoral coalition between the two parties.

While the simulation only attempted to estimate the representational consequences of this coalition by summing the votes received by the two parties in 1982 (and then undertaking the D'Hondt "highest average" calculations within each district), it is conceivable that such a combination might have produced more extensive electoral payoffs. The two groups might have complemented each other as they had in 1977 and 1979. Without Suárez, the UCD was led into the election by a candidate (Landelino Lavilla) lacking popular appeal—indeed, his mean feeling thermometer rating in the 1982 survey (3.3) was lower than that of Francisco Franco (3.8)! And without the UCD, Suárez's electoral efforts suffered from serious inadequacies in terms of provincial infrastructure and financial resources. Thus, he was an easy target for "wasted vote" attacks by rival parties. Perhaps even more important, the visibility to the general public of the traumatic schism of July 1982 damaged the personal reputations of leaders of both the UCD and CDS and undermined voters' confidence in them. We have demonstrated elsewhere that most voters who abandoned the UCD in 1982 cited the constant infighting and style of leadership of the party's elite as reasons for switching to another party (see Gunther 1986a, n.d. [a]). Thus, the centrist schism had a "multiplier effect" on parliamentary representation of parties of the center. It not only subjected the divided parties to the detrimental effects of the electoral law, but it also discouraged former UCD voters from supporting either the UCD or CDS out of disapproval of their squabbling behavior.

Caution should be exercised in interpreting simulations of this kind. There is no way of predicting how voters might have reacted to the prospect of casting ballots for a list including candidates of both parties. Contrary to the previous speculation, one might argue that such a ticket could have repelled some voters. (Indeed, when asked if there were any party or parties which our 1982 respondents would never support, 10% of those who voted for the CDS said that they would never vote for the UCD, and 6% of those who voted for the UCD said that they would never support the CDS). Thus, by simply adding together the votes received by each of these two parties, I am not taking into consideration possible defections from the coalition that might have resulted from dislike of the other party. On the other hand, as argued above, the votes that might have gone to a UCD-CDS coalition could have exceeded the total cast for them separately. I cannot say whether the net balance of these competing considerations would have had a positive or negative influence on support for this hypothetical coalition and have simply undertaken the simulation by adding together the two vote totals.

The results of our simulation suggest that such a coalition would have changed the outcome in 14 provinces. The alliance would have obtained one seat in each of 12 provinces in which neither party had received any in 1982, and in 2 other districts the coalition would have doubled the center's representation by picking up an extra seat. Thus, a combined UCD-CDS ticket could have won a total of 27 seats—over twice as many as the two parties running separately actually received in 1982 (11 for the UCD and 2 for the CDS) and three times the size of the AP's delegation in the previous parliament.

These net representational gains for a UCD-CDS coalition would have had a significant impact on the future prospects for the center and for the dynamics of partisan competition. First, since the hypothetical gains would have occurred mostly in provinces in which neither of the two parties actually received a seat, this political grouping of the center would have acquired a much broader geographical base. Second, the gains for UCD-CDS would have been substantial enough to allow them to form a parliamentary group, to survive, to reorganize, and to try again with perhaps better results in future elections. As the UCD's founding secretary general (who had correctly estimated the parliamentary representation of a UCD-CDS coalition at about 30 seats) said in a 1983 interview, "This would have left an embryonic parliamentary group with much better future prospects than the total disaster which ultimately occurred." Instead, the UCD subsequently disappeared, and the CDS delegation in the 1982–86 Cortes consisted of Adolfo Suárez and his brother-in-law.

The dynamics of party competition today might be different if such a coalition had materialized. Even though the CDS has survived what Suárez has called its "long march through the desert" and has subsequently increased its representation in the Cortes (electing 19 deputies in 1986), lingering hostility toward Suárez on the part of many of his former UCD collaborators (who blame him for contributing to the demise of the UCD) has impeded his efforts to build a credible centrist alternative to the PSOE. It should be noted, moreover, that even after four years of erosion of support for the incumbent Socialist government and after considerable efforts to reestablish a viable centrist party, the center in 1986 was represented in parliament by a delegation consisting of 8 fewer deputies than our simulation suggests it could have had in 1982.

Why did these party elites not respond to the incentives inherent in the Spanish electoral law by forming such a coalition? The overwhelming weight of elite interview evidence clearly reveals that all of the relevant party leaders were fully aware of this incentive structure, so one possible explanation can be dismissed. Instead, interviews relevant to this case study suggested that two other factors led to the abandonment of what otherwise might be considered a "rational" course of action: (1) other political goals were regarded as more important than the maximization of parliamentary representation in 1982, and (2) some key actors miscalculated their likely levels of electoral support and therefore did not fully appreciate how devastating the impact of the electoral law would be.

Evidence from various sources suggests that Suárez quit the party he had created primarily because he regarded a continuation of the status quo as unacceptable. His price for remaining in the UCD (as described in an interview with a key party leader) was that he should be granted authority to "rebuild the party from the ground up, drawing on those at the base of the party and eliminating previously established leaders—putting them in the 'fridge for a while." In a round of negotiations with Landelino Lavilla and outgoing prime minister Leopoldo Calvo Sotelo in July 1982, he proposed that Calvo Sotelo remove himself from positions of leadership within both the government and the party apparatus. According to the terms of his proposal, Suárez would become president of the party, with full control over the formation of electoral lists; Lavilla would be designated as the UCD's candidate for prime minister; and Calvo Sotelo and other "barons" of the party would step aside for a period of at least two years, with their places in parliament and government taken by a new generation of UCD leaders. This drastic surgery was necessary, in Suárez's view, in order to eliminate two sources of intraparty instability. The first of these was the de facto baronial structure of the UCD leadership, which made it possible for each individual faction leader to pursue, unchecked, "personal interests [and] personal ambitions for power." Suárez also believed that this loose structure of authority opened up the party to undue pressures from the *poderes fácticos* (the big banks, the Church, and the military) and undermined his ability to govern.

In the absence of a radical restructuring of the party's leadership, short-term electoral victory was regarded by Suárez and his collaborators as having little value. As one of them said in an interview, "What good is it to have 60, 70, 80 or 120 deputies, if we continue as we now are—if we elect one group favoring a pact with the PSOE and rejecting the discipline of the UCD, and another group favoring a pact with Fraga and rejecting the discipline of the UCD, and another group which threatens to quit if we don't give them three ministries? That's not a political party. We need to have lists which include persons who are disposed to accept party discipline. And if not, we will not have accomplished anything." Suárez echoed these sentiments in a published interview: "I have no confidence that, with so many personal political projects and so much political clientelism, UCD could have a coherent parliamentary group after the general election. . . . What can you do with ministers who, as soon as they leave the room, start to conspire against me, or phone the newspapers to tell them about private meetings we have just had?"[10] In short, Suárez set forth radical demands as the price of his continuation within the UCD in part as a reaction against the personal attacks to which he had been subjected over the previous two years and out of concern over continued ungovernability that might result from an unreformed party structure.

Not surprisingly, his demands were unacceptable to other party leaders. As one of the three participants in the July negotiations saw it, "Suárez wanted us to hand over the party to him, and hand it over to a dictatorial regime—with the collegial organs of the party, the executive, the Political Council all dissolved; that we would give full powers to him; that he would personally have powers that the statutes give to the executive and the Political Council; and that he could do, and undo, without being accountable to anyone until the following elections." Accordingly, this participant rejected Suárez's demands, and all possibilities of collaboration in the 1982 election were eliminated. In short, both Suárez and his adversaries in the UCD leadership regarded the basic structure of the party as more important than the prospects of improving their chances of winning seats in the 1982 election.

But uncertainty and miscalculations about likely levels of support for an independent CDS also confounded Suárez's decisions. A public opinion poll published on the eve of the Suárez-Calvo Sotelo-Lavilla negotiations suggested that Suárez and the progressive segments of the UCD might succeed in electing 55 deputies to the 1982 Cortes.[11] Given the accelerating decomposition of the UCD parliamentary delegation at that time, it would not have been unreasonable to extrapolate that substantial numbers of UCD deputies or provincial leaders might abandon that party in order to support a centrist *suarista* party. That same poll also suggested that such a party might play a pivotal role in the formation of future government coalitions: the PSOE was expected to win 159 seats in a new parliament and therefore would most likely need a coalition partner. Given Suárez's unpleasant prior experiences within UCD governments, a PSOE-CDS coalition government might have been an attractive alternative.

But Suárez miscalculated two important components of this strategy. First, he failed to take into consideration a loyalty to the UCD that was developing among several important potential allies. Very few members of the UCD elite followed Suárez into the CDS. Even persons who had been ideological allies, personal friends, and close collaborators refused to join the new breakaway party. The reluctance of progressive segments of the UCD to abandon their party was nicely summarized by a former party secretary general at the time of these critical decisions: "If we have traveled all over Spain for so many years, working for the UCD, defending the party and pleading for votes for it, how can we go to the same people now saying that it is worthless, and that they should vote for a different party?" Second (and perhaps as the result of the scarcity of prominent defections from the UCD), Suárez seriously overestimated the electoral prospects of a centrist party outside of the UCD. In light of these miscalculations, he underestimated the risks inherent in bolting from the UCD.

Less than one month after rejection of his demands by Calvo Sotelo, Suárez announced formation of his own separate party, the Centro Democrático y Social. And the rest is history.

The Gran Derecha

The second hypothetical coalition to be explored is one referred to in Spain as a "Gran Derecha" (Big Right) alliance. It joins the forces of the UCD with those of Alianza Popular. This combination would have fit with the long-standing desires of Manuel Fraga to combine the "centrist and center-right sectors, and set down the bases of a future party like the British Conservatives and the American Republican party." Indeed, the creation of a single large party that would attract support from all voters to the right of center was Fraga's original objective in founding Alianza Popular in 1976 (see Gunther, Sani, and Shabad 1986, chap. 3). With the outbreak of the intraparty conflicts that beset the UCD during the 2½ years preceding the 1982 election, Fraga was presented with an opportunity to restructure the party system at the expense of the heterogeneous center party. As a high-ranking AP leader explained in a 1981 interview, "Groups of citizens are working on important sectors of the UCD who have realized that those favoring divorce and those opposed, or those in favor of a collectivist fiscal policy and those opposed, can't be in the same party. We're having a new look at those conservative and Christian democratic groups." Throughout the year preceding the 1982 election, AP leaders redoubled their efforts to encourage defections of individual UCD politicians (such as the centrist party's former floor leader, Miguel Herrero de Miñón) or outright schisms (such as the one that culminated in the creation of the Christian democratic Partido Demócrata Popular [PDP]) that might facilitate a broader coalition of center-right forces. And on the very eve of that election, they entered into discussions concerning a formal coalition between the AP and what remained of the UCD.

This strategy was compatible with the political preferences and calculations of some UCD leaders. It also fit with their perceptions of the impact of the Spanish electoral law on the nature of the party system. As the future founder of the PDP stated—somewhat inaccurately—in an interview in 1981 (when he was still a UCD deputy),

> We have an unusual electoral law in Spain. There are 50 provincial districts, the majority of which are small, agrarian districts, which are overrepresented by up to three to one: one vote in Segovia or Avila is worth three in Madrid. In these districts Alianza Popular usually has considerable support, but not quite enough to elect a deputy, because these are districts which elect three or four deputies. If you were to study the election results and add the votes of AP to ours, in three-quarters of the provinces we would pick up an extra deputy at the expense of the left. The benefits of forming an electoral coalition with Alianza Popular are obvious.

In the weeks preceding the presentation of lists for the 1982 election, there was talk of a possible coalition between the UCD and AP—a ticket that Manuel Fraga argued would appeal to a "natural majority" of Spaniards. Such an alliance had been favored for some time by certain leaders of the conservative sector of the UCD's Christian democrats and by certain other conservative members of the party elite. In fact, some had already left the party in order either to join the AP directly (e.g., Herrero) or to move into coalition with the AP within Oscar Alzaga's new PDP. Others who remained inside of the UCD (such as José Luis Alvarez) pushed for a preelection alliance with the AP on the eve of the 28 October election. The debate over this proposed alliance came to a head at a Madrid meeting of the party's leadership. On 7 September, the UCD rejected those proposals and made a "solemn public proclamation" of its intention to stand alone. Some UCD leaders (such as José Luis Alvarez) resigned from the party in protest and joined the AP-PDP coalition.

Our simulation of this electoral alliance suggests that a UCD-AP-PDP coalition would have won 14 more seats than they actually secured fielding separate slates of candidates. The usual caveats surround simulations of this kind, but these findings suggest that the UCD's prospects for survival might have been considerably enhanced if it has entered into such a coalition. Instead, the mechanical effects of the electoral law substantially reduced its delegation in the Cortes, and the reinforcing biases of campaign-financing formulas drove it to bankruptcy.

What explains the UCD's refusal to enter into such a coalition? Interviews with the protagonists in this drama and public statements by them suggest that five factors lay behind their refusal to adopt what the electoral law would have defined as rational behavior, insofar as they put maximizing representation above other goals. The first is that such a coalition would have represented an abandonment of one of the central beliefs and commitments of the UCD—that a powerful and independent centrist party is necessary in Spain because a bipolar party system would have divisive and potentially destabilizing consequences for the regime. Second, personal animosities and suspicions towards Manuel Fraga and his conservative party were widespread. Third, such a coalition would have clashed with the ideological heterogeneity of the UCD elite and with the progressive inclinations of many of its leaders. (These latter two points are revealed in a 1983 interview with a former UCD secretary general, who responded, when asked about such a coalition, "I wouldn't even *vote* for Fraga! I have a much greater fear of Fraga than of Felipe—lots more. I know that Felipe is a democrat. About Fraga, I'm not so sure. I don't feel the least bit identified with the right in Spain, because it is an unmodern right.")

A fourth factor was based on a tactical consideration: by September 1982 public opinion polls had indicated that the UCD would have to be a junior partner in such a coalition, and this had unacceptable implications for many party leaders. Another former secretary general of the party pointed out that "it was AP that proposed the idea of a coalition. Alianza Popular is in the driver's seat, and it is convinced that it should be the principal protagonist and director of such a coalition. We would be under the leadership of Mr. Fraga. No! That would represent the disappearance of the UCD and the disappearance of a political space that the CDS could not fill by itself."

Finally, uncertainty concerning actual levels of electoral support for each of the prospective coalition partners undermined the persuasiveness of this coalition proposal. Many UCD leaders firmly believed that under Fraga, the AP could never break through an electoral ceiling of about 20% of the vote. This, coupled with poll data indicating that a substantial segment of the electorate had still not made up its mind one month before the election, led some to conclude that the electoral advantages of an alliance with AP would not be as favorable—and the electoral prospects of a UCD standing alone might not be as bad—as they ultimately proved to be.

Again, party elites failed to behave consistently with the incentives inherent in the electoral law as a result of uncertainty about their likely levels of electoral support and as a by-product of weighing other political concerns and objectives more heavily than considerations of short-term electoral advantage.

Fragmentation on the Right

The third case study is very different from the two that have preceded it: it is an exploration of the rewards and disadvantages of an electoral coalition that was actually formed in advance of the 1982 parliamentary election. The history of the alliance among the Christian democratic PDP (formed in 1982 as the result of a schism within the UCD), Alianza Popular, and two tiny liberal groups (the Unión Liberal in 1982 and the Partido Liberal [PL] in 1986) gives further evidence of the complexity of the trade-offs that must be taken into consideration in forging electoral coalitions. It also indicates why party leaders should exercise extreme caution in entering into such agreements.

Three reasons were set forth by AP and PDP elites when asked to explain why they had formed the Coalición Popular (CP) in 1982. The first is that they were anticipating the mechanical and psychological effects of the electoral law, and hoping that the votes attracted by the newly formed PDP would be sufficient to substantially augment the representation of conservatives in the Cortes. In this respect, the AP-PDP-PL coalition was a somewhat truncated version of the Gran Derecha strategy that Manual Fraga had been advancing for several years. As is usual when such agreements are made, much uncertainty surrounded the likely levels of electoral support of each prospective coalition partner. The PDP (in its efforts to negotiate for itself a large number of favorable positions on electoral lists) decidedly overestimated its electoral clientele, and succeeded to some extent in convincing AP leaders that it might play a decisive role in expanding the parliamentary presence of the Right.

A second reason set forth is that the two sets of elites believed that they had complementary but distinct potential electorates and that this might further expand their combined electorate. They believed, for example, that centrist to center-right Christian democrats who were not particularly attracted to AP might be induced to vote for a coalition that included a party like the PDP. At the same time, they argued that individuals who might never risk "wasting" their votes on a small party like the PDP might support a broader coalition of the center-right.

The third rationale was largely historical, focusing not so much on what the PDP might contribute in future elections as on how decisive it had been in restructuring the party system in 1982. As an AP provincial leader claimed in an interview, the PDP was not so much an electoral party as "an operation designed to destroy the UCD. That's how the PDP was born; with clear intentions of allying itself with the AP. The PDP was born with the assistance of the AP in order to finally break the UCD. The PDP was intended to be the gathering place for those disenchanted with the UCD." In this latter respect, the AP alliance with the PDP was certainly successful. Giacomo Sani and I have demonstrated elsewhere (see Sani 1986; Gunther 1986a, n.d. [a]) that the dramatic electoral gains of the AP in 1982 were at the expense of the UCD and that the principal factor in the collapse of electoral support for the UCD was the unseemly decomposition of its elite. But in other respects, the coalition can be seen to have had decidedly detrimental consequences for Alianza Popular.

For one thing, the mechanical effect of the electoral law produced a negligible increase in the parliamentary representation of the Right as a product of this coalition. Sani and I simulated the 1982 election with the AP, PDP, and liberals fielding separate slates of candidates, using as the estimate of the size of the respective vote shares of each party the votes they actually received in the 1987 municipal elections—when they fielded separate slates of candidates. (This simulation entails additional caveats beyond those set forth above: one cannot be sure to what extent electoral support for each party was equivalent to the actual levels of support they received 4½ years later—although public opinion poll data suggest that no significant change in support for these parties occurred during this period). The actual levels of electoral support received by the PDP and PL in the 1987 municipal elections were abysmally low: nationwide, the PDP received only about 150 thousand votes, and the Partido Liberal received fewer than 11 thousand.[12] By way of comparison, Coalición Popular received over 5.2 million votes in 1982. Accordingly, the simulation of the 1982 election with the three parties running separately reveals that the PDP contributed a net of two seats to the conservative parliamentary delegation (the CDS and PSOE otherwise would have each gained one seat) and that by the narrowest margin, the PDP would have elected only one deputy of its own. This would have had a minimal impact on the balance of partisan forces in the Cortes.

From the standpoint of the AP, the formation of Coalición Popular actually led to a substantial reduction in Cortes representation. The AP standing alone in 1982 would have elected 103 deputies if our simulation can be believed. Instead, only 83 AP candidates were awarded seats in the Congress of Deputies. This is because the AP's coalition partners—particularly the PDP and the liberals—were generously rewarded by Fraga in allocating favorable positions on electoral lists. Despite its meager electoral contribution to the Coalición Popular (I estimate that the PDP's share of the CP vote was only about 3% of the total), Oscar Alzaga's Christian democrats were given 16 seats in the Congress of Deputies in 1982, or about 15% of the total CP delegation. Liberals associated with the virtually nonexistent Unión Liberal were given a half-dozen seats in the Congress. In the 1986 general election, the PDP and the liberals did even better. Alzaga's Christian democrats were given 21% of all of the electoral list positions nationwide (as compared with 11.5% for the Partido Liberal and 67.5% for the AP), and it won 22 seats in the Congress, or 21% of the CP parliamentary group. Even more startling, the Partido Liberal (which—remember—received only 11 thousand votes nationwide in the following year's municipal elections!) elected 12 deputies, or 11% of all Coalición Popular seats. As a result, instead of a parliamentary delegation of over 100—which Alianza Popular would have had if it had fielded a single-party slate of candidates—AP was represented by only 71 deputies. Fraga's generosity toward his coalition partners was bitterly resented by AP regulars and gave rise to considerable intraparty conflict in some regional branches.

Over the long term, the formation of Coalición Popular can be seen to have had even more negative, if not disastrous, consequences. This was the result of rancorous squabbling that erupted in the aftermath of the 1986 election. From the beginning, there had been concern within the AP elite over the consequences of close affiliation with the PDP. The prominent roles in the destruction of the UCD played by the PCP's secretary general and president (Alzaga and Alvarez) led some AP leaders to treat them with extreme suspicion, even to the point of opposing any possible merger between the two parties. When asked why there had been no formal merger between the AP and the PDP, Alianza Popular's secretary general said in a 1983 interview, "I know too well the history of the UCD to accept them within Alianza." He went on to portray Coalición Popular as a naval fleet, with AP as the flagship: "First, there is the aircraft carrier—AP. Next, there are two or three *destroyers* which are perfectly fine staying where they are. If the destroyers want to form part of the fleet, fine. But they will not intervene in our internal matters!" Another AP official added, "They are allied with us, but very opportunistically. They're allied with us only as long as they need us."

The events that transpired over the following years suggest that these suspicions were well founded. In the course of the very first press conference following the 1986 election (in which Coalición Popular lost two seats, largely as a result of the defection from the coalition of two small regional parties), PDP leader Alzaga launched an attack on Manuel Fraga, arguing that unless the coalition were led by a new political figure, it would never win an election. Within a few short months, the CP broke down, and leadership struggles erupted within the component parties. And within less than a year, both Fraga and Alzaga were forced to resign as leaders of the parties they had founded. Alianza Popular was plunged into a crisis from which it has not yet fully recovered. Thus, one of the great assets forged by Manuel Fraga and his collaborators following the schism of 1978—a unified and stable leadership—was destroyed in the acrimonious aftermath of a mild electoral disappointment for Coalición Popular. The breakup of the Coalición Popular delegation in the Congress of Deputies (with the entry of both the PDP and the PL into the mixed group) has resulted in not only an increase in partisan fragmentation in parliament (which the electoral law was explicitly designed to discourage) but also in an increasing imbalance of partisan forces between the incumbent Socialists and the largest party of opposition.

Conclusions

These simulations and explorations of elite-level coalition-building strategies reveal that the implications of the Spanish electoral law (and electoral laws in general) for the dynamics of partisan competition are complex and multifaceted. To be sure, some aspects of the influence of electoral laws are simple and direct: how the law translates popular votes into parliamentary seats is straightforward and mechanical; and sophisticated or strategic voting by Spaniards has become so widespread and predictable that *voto útil* appeals have regularly been incorporated into campaign strategies of large parties since the 1979 election (see Gunther, Sani, and Shabad 1986, chap. 8). But the distal effects, or long-term political consequences, of electoral laws involve complex interactions at several levels within the political system.

In order to assess the role of electoral laws in shaping the nature of partisan competition and coalition formation properly, it is important to start with the simple notion that this device creates a predictable structure of rewards and disadvantages for political parties. Other things being equal, party leaders, functioning as rational actors, should behave in a uniform and predictable fashion. But "other things" are not equal, and as a result the preceding analysis suggests that we alter our understanding of the distal effects of electoral laws on party systems in the following ways:

1. Electoral laws should be regarded as "*only one* of the causal elements" that determine the structure of party systems (Duverger 1986, 71).
2. The distal, or long-term, impact of

electoral laws on party systems is best regarded as mediated through the behavior of party leaders, involving such matters as the formation of electoral coalitions, mergers with former rivals, and intraparty schisms.

3. Electoral laws influence elite behavior in these areas by presenting to party leaders a structure of incentives involving rewards (e.g., increases in parliamentary representation and monetary compensation from public financing of election campaigns) and punishments (e.g., underrepresentation or outright denial of parliamentary representation, and even bankruptcy).
4. The impact of electoral laws on these aspects of elite behavior would be direct and deterministic only if elites
   a. have full knowledge of the incentives inherent in the electoral law
   b. have reasonably accurate knowledge of the levels and geographical distribution both of their own bases of voting support and their prospective coalition partners'
   c. place the maximization of their party's parliamentary representation in the short term over all other political objectives

An examination of party elite behavior in Spain in the early 1980s has revealed several instances where one or more of these conditions were not met. First, while nearly all party leaders were fully aware of the representational biases inherent in the Spanish electoral law, some key actors either had inaccurate information regarding their likely electoral support or intentionally misrepresented their levels of electoral appeal in the course of bargaining with potential coalition partners. This seriously distorted decisions regarding the formation of electoral coalitions and produced outcomes that were "irrational" if one regards maximization of parliamentary representation as the rational objective.

More importantly, a significant number of key elites challenged the primacy of this objective and placed other concerns above the short-term maximization of parliamentary representation. Among those other concerns were personal animosities; the desire to respect the traditions of their party and the integrity of the party institution itself; perceived ideological or programmatic incompatibility with potential coalition partners; concern over how certain party system configurations might threaten the stability of the democratic regime; and the desire to avoid repeating the mistakes of the past.

As a result of uncertainty, miscalculations, or greater concern for matters other than with short-term maximization of parliamentary representation, these elites engaged in behavior that substantially departed from what the electoral law would have determined to be rational. Thus, even though the broad outlines of the party system clearly accord with the biases of the electoral system, certain of its features remain as otherwise unexplainable residuals. Moreover, some of these departures from the above assumptions provide insights into why an electoral law designed to facilitate party system simplification and stability paradoxically contributed to a tumultuous and destabilizing electoral crisis in 1982.

The level of support likely to be received by each individual party is always uncertain up to the moment when ballots are actually cast. While the publication of poll data has subtantially reduced this uncertainty in recent years, under some circumstances this uncertainty may still be considerable. In the analysis presented above, I have shown that leaders of the PDP skillfully manipulated uncertainty concerning the size of their potentially supportive electorate, incorporated arguments linking this level of support to the known biases inherent in the electoral law, and succeeded in carving out for themselves a much larger share of Coalición Popular's parliamentary delegation than would otherwise have been justified. The simulation presented above supports arguments set forth by some AP partisans that the interests of the party were not well served by the formation of Coalición Popular. This is true both in the short-run (by allocating to the AP a significantly smaller number of seats in the Cortes than it would have won on its own) and especially in the long run (by exposing the previously stable AP elite to the same kinds of disruptions as had destroyed the UCD). In short, uncertainty, combined with awareness of the representational biases of the electoral law, led the AP to engage in behavior that was not in keeping with rational calculations of the self-interest of the party. Uncertainty also contributed to the initial electoral disaster experienced by Adolfo Suárez and his CDS. Overestimating his personal electoral appeal and his ability to encourage additional defections from the UCD leadership, Suárez and his party stood alone in the 1982 election and were nearly wiped out as a viable political force.

Other portions of the analysis set forth above suggest that the maximization of votes over the short term may be outweighed by other considerations, leading party leaders to engage in behavior that departed from the structure of rewards and punishments inherent in the electoral law. The simulations above clearly indicated that the centrist parties would have maximized representation (and that the UCD, in particular, might have survived the 1982 election) if they had avoided schisms, formed electoral alliances, or merged. Instead, they acted in a manner contrary to this logic, and, divided, they fell. Why? Several reasons were set forth in elite interviews suggesting that other concerns outweighed simple calculations of short-term electoral advantage. With regard to every possible combination, personal animosities precluded an alliance: either deep-seated suspicions had taken root over several years (e.g., the hostility of many UCD elites towards any possible alliance with Fraga), or scars from recent battles were too fresh to have healed (e.g., between Suárez and several UCD leaders). Concern over ideological compatibility and lingering suspicions over the AP's commitment to democracy represented a second set of considerations that precluded an AP-UCD coalition. A third factor, which appears to have been decisive in preventing the UCD and CDS from reestablishing a collaborative relationship, involves models of party organization and the general desire to avoid a replication of the mistakes of the past: Suárez refused to remain in a party within which powerful "barons" could retain considerable autonomy and within which party discipline was weak; and Calvo Sotelo refused to return control of the party to an individual who he feared was potentially dictatorial. All of these elite concerns clash with the logic inherent in the representational biases of the electoral law.

These findings lead inexorably to another set of issues—concerning the institutionalization a new political party—that cannot be explored here (see Gunther 1986b, n.d.[b]). The incentives inherent in electoral systems generate payoffs primarily at the level of party-as-institution. This presupposes that individual leaders within those parties will coordinate their actions in the interest of maximizing representation for the party as a whole, which may require that they occasionally sacrifice their own individual or factional interests. This may be difficult to achieve in the absence of a widespread commitment to the party in the abstract, particularly when such matters as internal party governance and discipline are still open to challenge. For this reason, the structure of incentives presented to elites in new political parties in Spain may have been less compelling than that facing more fully institutionalized parties. Thus, the degree of party institutionalization should be regarded as yet another set of intervening factors that mediates between electoral laws and the structure of party systems. And as long as analysis of the distal impact of electoral laws on party systems is conducted at the high level of abstraction typical of most of this literature, the roles played by these intervening factors cannot be adequately assessed.

Notes

1. The two surveys (N = 5,439 and N = 5,463) were conducted by Giacomo Sani, Goldie Shabad, and me were partially or fully financed by the National Science Foundation under grants nos. SOC 77-16451 and SES-8309162. (The opinions, findings, and conclusions herein, however, are those of the author and do not necessarily reflect the views of the National Science Foundation.) The 1982 survey was undertaken (in collaboration with Juan Linz, José Ramón Montero, and Hans-Jürgen Puhle) with generous support from the Stiftung Volkswagenwerk of West Germany.

2. A preliminary version of this paper was coauthored with Giacomo Sani (Sani and Gunther 1986). The author is grateful to Sani for permission to use those simulations in this substantially revised version.

3. Approximately two hundred extensive in-depth interviews (usually lasting over an hour each) were conducted with party leaders in 1978, 1979, 1981, 1983, 1984, and 1986. Those interviewed included leaders of all five of Spain's nationwide parties (the PSOE, UCD, PCE, AP and CDS) and the major regional parties (the Catalan CDC and ERC, and the Basque PNV and Euskadiko Ezkerra). This series included a total of 11 hours of interviews with Spain's three democratic prime ministers.

4. The campaign-financing law enacted in March 1977 grants public subsidies to parties in accord with the number of seats won in the preceding election and for each *voto útil* (i.e., votes for *successful* candidates). Parties failing to elect a single deputy or senator receive nothing. In short, the rich get richer, and the poor are driven to bankruptcy. Similarly, the December 1978 law that reimburses parties for their annual operating expenses works to the detriment of smaller parties: public funding is granted to each parliamentary group and for each elected deputy and senator. See Castillo 1986.

5. See Abbreviations.

6. The "limited vote" system adopted in 1931 facilitated the proliferation of independent parties and produced a highly fragmented party system. In each of the three legislatures of the Second Republic more than 20 parties received parliamentary representation, with at least 11 parties in each session having 10 or more seats. In none of those legislatures did a single party control more than 23% of the seats. This meant that broad-based multiparty coalitions were required for the formation of a government. As one might expect, this led to considerable cabinet instability: during the 25 months of the 1933–35 Cortes session, for example, there were 13 government crises. See Gunther, Sani, and Shabad 1986, 17–19.

7. It is important to note that a high score on the Index of Proportionality does not necessarily imply that an electoral system is "weak" (i.e., that it exerts little influence on the shape of the party system). This measure of the mechanical effect of the electoral law does not take into consideration the distal effect of an electoral system, which may be so powerful as thoroughly to discourage potential competitors from entering a race under unfavorable conditions. The United States, for example, has a high score at least in part because of the lack of third-party challenges to the two-party system, which the U.S. electoral law had helped to bring about (Duverger 1954, 218–20).

8. *Cambio 16*, 27 September 1982. p. 191.

9. The province of Avila, where Suárez's "favorite son" status meant that his party would receive considerable support (and therefore would not be punished by the electoral law), has been excluded from the "small provinces" category.

10. Antxon Sarasqueta, "El Duque se fue del buque" [The Duke abandons ship], *Cambio 16*, 9 August 1982, p. 18.

11. Antxon Sarasqueta, "La movida de la derecha" [Movement on the right], *Cambio 16*, 28 June 1982, p. 25.

Abbreviations

AP	Alianza Popular (conservative, Manuel Fraga)
CDC	Convergència Democràtica de Catalunya (Catalan)
CDS	Centro Democrático y Social (center, Adolfo Suárez)
CG	Coalición Galega (Center, Gallego)
CP	Coalición Popular (center-right)
CIU	Convergència i Unió (Catalan nationalist, Jordi Pujol)
EE	Euskadiko Ezkerra (left-wing Basque nationalist)
ERC	Esquerra Republicana de Catalunya (Catalan)
ETA	Euskadi ta Askatasuna (revolutionary Basque nationalist)

HB	Herri Batasuna (extreme left pro-ETA Basque nationalist)
ORT	Organización Revolucionaria de Trabajadores (extreme left)
PCE	Partido Comunista de España (Eurocommunist, Santiago Carrillo)
PDC	Pacte Democràtic per Catalunya (Catalan nationalist, Jordi Pujol)
PDP	Partido Demócrata Popular (Christian democratic, Oscar Alzaga)
PL	Partido Liberal (liberal)
PNV	Partido Nacionalista Vasco (Basque, center-right)
PSOE	Partido Socialista Obrero Español (socialist, Felipe González)
PSP	Partido Socialista Popular (socialist, Enrique Tierno Galván)
PTE	Partido del Trabajo de España (extreme left)
UCD	Unión de Centro Democrático (center to center-right, Adolfo Suárez)
UN	Unión National (extreme right, Blas Piñar)

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MODERNIZATION AND PARTICIPATION IN THE LENINIST DEVELOPMENTAL STRATEGY

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The Soviet Union achieved its stability in the early stages of development not by institutionalizing participation but by forcing departicipation and substituting a functionally distinct form of political activity—involvement in coproduction. These policies constitute essential complements in the Leninist developmental strategy. The ability of enlisted involvement to block the growth of participatory pressures tends to decline in later stages of development, however. The result is spontaneous withdrawal from the institutions of coproduction and the rise of participatory pressures. This pattern is documented with evidence from Soviet electoral and membership statistics, Soviet reports of opinion surveys, Western interviews of Soviet émigrés, and cross-national estimates of political dissidence. The Leninist crisis of participation requires a policy response to close the participation-institution gap once again. The alternative policy responses of Soviet general secretaries are characterized as totalitarian, authoritarian, liberal, and socialist.

Among the accomplishments of the Soviet Union has been its uncommon stability during transitional stages of political modernization. Huntington (1968, 335) attributes this to "one of the most significant political innovations of the twentieth century"—Lenin's "theory explaining how to organize and to institutionalize the expansion of political participation which is the essence of revolution." But the Leninist pattern of development pioneered in the Soviet Union did *not* include the early institutionalization of political participation, though it did sustain political stability. As defined in Huntington's analysis, participation is "activity by private citizens designed to influence governmental decision-making." It may be autonomous if designed by the actor or mobilized if designed by someone other than the actor. But regardless of who instigates this activity, it "has to be directed at, and have an impact on, the loci where decisions are made" (Huntington and Nelson 1976, 4, 7, 50). No such activity was institutionalized in the Soviet Union (Jowitt 1983, 282). Rather, as the new Leninist regime confronted extensive political participation at the time of the revolution, its success was achieved by actually suppressing public participation in decision making and in selecting decision makers. It avoided the slide into praetorian politics in large part by institutionalizing an alternative form of nationwide political activity. Specifically, the genius of the Leninist pattern of political development as practiced in the Soviet Union prior to Gorbachev has not been to expand and institutionalize participation

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but to manage successfully the forced departicipation of society by substituting involvement in coproduction.

Although this argument rejects Huntington's (1968, 80) categorization of the Soviet Union as a participant political system, revises his analysis of the Leninist strategy, and argues that his analysis overlooks a significant alternative to institutionalization of participation, it nonetheless accepts Huntington's analysis as a useful point of departure for placing the Leninist developmental experience in a comparative perspective. The Leninist path of political development may not in the long run be able to escape the imperative to institutionalize expanding participation. The initial Leninist response to the crisis of participation, by suppressing participation and leaving the institutionalization of participation incomplete, does not necessarily resolve that crisis but, in fact, may simply postpone it.

The Huntington Thesis

The institutionalization thesis builds on empirical studies that find a curvilinear relationship between modernization and political instability; that is, instability tends to reach its highest levels at intermediate stages of mobilization and economic development. In the final analysis, however, it is the balance (or gap) between *political* development and the processes of social and economic change that yields stability (or instability) in transitional societies (Huntington 1968, 39–56).

In Huntington's analysis, social mobilization (S) gives rise to "enhanced aspirations and expectations which, if unsatisfied, galvanize individuals and groups into politics." These aspirations and expectations may be fulfilled outside political action through successful economic development (E) that increases the capacity of society to satisfy aspirations or through the presence of opportunities for social and economic mobility (M). If not satisfied through these means, "political participation becomes the road for advancement" (P). Under these conditions social frustration leads to the expansion of political participation to enforce demands on government. In reduced form, Huntington presents this relationship as a conceptual equation: $P = S/(E + M)$. Political participation differs from other means of satisfying aspirations and expectations in that it seeks this by influencing governmental decision making (Huntington 1968, 49, 55).

Political stability in the face of this political modernization requires political development—measured by "the creation of political institutions sufficiently adaptable, complex, autonomous, and coherent to absorb and to order" expanding participation. Conversely, political instability is "in large part the product of rapid social change and the rapid mobilization of new groups into politics coupled with the slow development of political institutions" (Huntington 1968, 4, 47, 55, 89, 315).

Leninist regimes have been unusually adept at institution building, according to Huntington (1968, 8, 335–41): "The distinctive communist achievement has . . . been the creation after revolutions of modern governments based on widespread mass participation in politics." In this view, communists actually seek to expand political participation and bring this mobilized population into their institutions: "Only the communists have consistently demonstrated the ability to organize and structure that participation and thus to create new institutions."

The Limits of Leninist Institutionalization

Contrary to Huntington's analysis, the political activity that the Leninist polity has typically sought to institutionalize is not participation (nor is it simply sham participation, as others argue) but a functionally distinct form of political activity—involvement in coproduction. The distinction between this form of political activity and participation, consistent with Huntington's definition of the latter, is their relationship to the decision making process: Political participation is activity to influence governmental decision making; coproduction is the implementation of decisions already made.

The distinction between involvement in coproduction and political participation is an important comparative concept, for political systems are distinguished in part by the extent to which they institutionalize each. The Leninist pattern of political development pioneered in the Soviet Union is associated with the interlocking and interdependent strategies of forced-draft expansion of popular involvement in coproduction and constriction of participation (Little 1976, 1980; McAuley 1984; Sharlet 1967; Tucker 1961, 285). The stability of the Leninist regime has depended on involvement in coproduction to provide the socioeconomic (*E*) and mobility (*M*) incentives that obviate the growth of participatory pressures (*P*). Yet the limits to this Leninist strategy may be manifest in a later crisis of participation.

Coproduction is the joint production of public goods and services by the governmental bureaucracy and citizen volunteers (Mattson 1986, 51). Long the dominant form of citizen involvement in Leninist polities—and described as the buzzword of public managers in the United States in the 1980s—involvement in coproduction has drawn little attention from students of comparative politics as a distinctive form of citizen political activity. It typically takes the form of volunteers working with administrations or through auxiliary organizations established as satellites or extensions of governmental bureaucracies. For analytic precision, coproduction should be distinguished from all paid employment, including part-time employment or private contracts, and from provision of services by the private sector due to a narrower definition of the public domain or the abdication of functions by government. Moreover, it refers to activities or behavior designed to implement policy, so that simply being affected by policy or complying with policy (e.g., paying taxes) does not constitute involvement any more than law-abiding residence in a country constitutes participation in a political system (Huntington and Nelson 1976, 4–6, 10; Warren, Rosentraub, and Harlow 1984, 449).

The distinction between political participation and involvement in coproduction requires three further (definitional) clarifications. First, it requires a strict distinction between the structural location of political activity and its function. Specifically, efforts to influence governmental officials, whether at the point of general policy formulation or at the point of implementation (e.g., particularized contacting) constitute participation; only actual implementation constitutes involvement in coproduction. Second, it requires a strict distinction between the forms of activity and their function in the policy process. According to Huntington and Nelson (1976, 12–13), the forms of participation include electoral activity, lobbying, organizational activity, particularized contacting, and violence. Yet the function of political activity is determined as much by the context in which the act is performed as by the form of the act itself (Przeworksi and Teune 1970, 103–4). Thus, in Leninist polities many of these forms typically mask involvement in coproduction—specifically, a subset of involvement that Brzezinski (1969, 20) has labeled "pseudo-participation" (Burks 1982, 141–50). Other forms of involvement in coproduction are as diverse as the policy outputs of a political system (e.g., volunteer firefighting) but in a Leninist polity may be institutionalized as the *druzhiny* (volunteer militia), *remdruzhiny* (volunteer repair brigades), and *subbotniki* (unpaid public labor). Third, some political acts may perform more than one function and so partake of both participation and involvement in coproduction.

Although political systems may typically be characterized by a combination of participation and involvement, a key distinction among traditional political systems, modern liberal polyarchies, and Leninist polities has been the extent to which each form of political activity is institutionalized. In primitive societies the functions of decision making and implementation may be fused (Mair 1977). In many later developing societies, where the growth and professionalism of the state administration have not kept pace with the demands of expanding policy outputs and where service obligations to the state survived until recently, involvement in coproduction may be integral to the conception of political activity. With the expansion and professionalization of state administration and with the demise of service obligations at the end of the feudal era, however, this form of activity has been relatively less common in advanced industrial democracies (Brudney 1986, 57; Weber 1946). The liberal concept of governmental responsiveness leaves less room for service obligations. And professional administrators have successfully resisted sustained (institutionalized) intrusion into their activities from nonprofessionals—except through the formal channels of hierarchical subordination and democratic accountability (Warren, Rosentraub, and Harlow 1984). In the Leninist model of development, however, involvement in coproduction is central to the conception of citizen activity; indeed, this developmental strategy has sought to substitute involvement for participation.

Leninist theory itself speaks of initially reestablishing the fusion of involvement and participation during the early stages of the transition to communism. As Lenin notes in *State and Revolution* (1962a, 116), "Under Socialism much of the 'primitive' democracy is inevitably revived, since, for the first time in the history of civilized society, the *mass* of the population rises to *independent* participation, not only in voting and elections, *but also in the every-day administration of affairs.* Under Socialism, *all* will take a turn in management, and will soon become accustomed to the idea of no managers at all." Yet the developmental objective has been (at least until Gorbachev) to substitute this self-administration for any participatory process involving competition of interests: as the USSR Soviet of the Union chairman Tolkunov (1986, 85) summarizes, "We believe that the functions of self-governance consist not in the working people being able to make demands on enterprise management and the state as a whole, but in joining them in helping their collective to achieve success and the society to flourish." Involvement in coproduction is to grow as *"every* toiler, having finished his eight hours' 'task' in productive labor, shall perform state duties *without pay"* (Lenin 1962b, 204). At the same time, with the movement toward communism politics will disappear, decision making will become scientific choice, and citizen participation will be superceded entirely by involvement in coproduction. It is instructive that when identifying a present practice that would prevail under communism, Lenin (1963a, 36) chose a form of involvement: "If there is anything communist at all in the current system in Russia, it is only the *subbotniki*, and everything else is nothing but the struggle against capitalism for the consolidation of socialism" (Gilison 1975, 109–11; Kubadinsky 1983; Lazarev 1982; Stanovkin 1981).

Forced Departicipation

Coming to power in a hostile, politically mobilized environment, half the equation of Leninist stability was the successful exclusion of the previously participant population from politics (Jowitt 1975, 70, 81). The new Soviet regime expressed this symbolically through the disenfranchisement of the participant classes (Unger 1981, 37). More practically, this has been accomplished through their liquidation, demobilization, intimidation, and reeducation.

What gives coherence to these policies is their systematic attack on the bases of participation. Huntington and Nelson (1976, 93–94) argue that there are "two distinct channels leading to higher levels of political participation." One path leads to higher levels of participation by way of higher socioeconomic status and "increased subjective feelings of political efficacy," the other by way of group consciousness and organizational involvement. Leninist policies forced departicipation by assaulting the objective and subjective bases of the status and organizational paths to participation.

Liquidation (physical removal from the political system)—particularly through incarceration or simply by killing—stripped the participant stratum from the new Leninist society. The victims of the *Cheka* were disproportionately drawn from the participant population (e.g., officials of the old regime, political activists) and from groups that by their socioeconomic status were identifiable as potential participants (e.g., former nobles, bourgeoisie, landowners, and clergy) (Fainsod 1967, 426–30).[1]

Demobilization (removing the organizational bases for unofficial political action) may be the most important means to departicipation; for as Huntington and Nelson (1976, 85) point out, "An increasing body of evidence suggests that organizational involvement may be more important than socioeconomic status in explaining differences in political participation" (Perlmutter 1981, 24–28, 180). Demobilization eliminates independent organizations or the independence of organizations (Schapiro 1977; Selznick 1952). It is more limited in objective than atomization but in extreme forms may produce this (Dallin and Breslauer 1970, 49–55; Moore 1954, 415–17).[2]

These Leninist strategies force departicipation by removing the objective bases of participation. But participation also involves subjective feelings. Intimidation, which may be achieved through terror, deters from political action members of the participant population that have not been physically removed. Among the purportedly "unique" characteristics of terror in communist systems is its extension beyond "actually or possibly culpable individuals . . . to engulf entire social groups" (Dallin and Breslauer 1970, 7, 32, 34; Linz 1975, 218). This social prophylaxis takes on special meaning in the context of the new Leninist regime's crisis of participation—forcing departicipation by intimidating the larger population with the education, skills, psychological orientations, and organizational ties essential to participation. Reeducation of the socially mobilized population—in its extreme form, thought reform as practiced in China—may force departicipation by manipulating the subjective bases of both individual political efficacy and group consciousness (Chen 1960, 9; Lifton 1961, 5).

Enlisted Involvement

Even as the new Leninist regime sought to destroy independent participation, it filled the potential void with a new associational life not only of sham participation but also of involvement in coproduction. Lenin wrote in *The Proletarian Revolution and the Renegade Kautsky* (1963b, 256), "The Soviet government is the first in the world . . . to enlist the people, specifically, the exploited people, in the work of administration." According to Kenez (1985, 254), "A characteristic of Soviet life in the 1920s was the abundance of movements, societies, and organizations, in which each had the task of advancing one or another goal of the regime." This involvement in coproduction has been institutionalized in nonstaff (*vneshtatnyi*) departments within administrations; in a dense network of "transmission belts" or organizational adjuncts to administrations such as trade unions, residential committees, and public organizations; and in the rituals of pseudoparticipation (Friedgut 1979, 248; Lee 1979, 21).

Coproduction can grow to involve all the major bureaucracies and major policy functions of the Leninist party and state. Foremost, coproduction extends the reach of the Party's agitation-propaganda apparatus and the state's educational bureaucracy. The intertwined functions of technical education, political instruction, and implicit socialization are central to the activities of volunteer agitators; membership in the trade unions and largest public organizations, such as the All-Union Knowledge Society (Znanie) and the All-Union Volunteer Society for Cooperation with the Army, Air Force, and Navy (DOSAAF); and much pseudoparticipation. For example, Soviet electoral campaigns, according to Friedgut (1979, 137–47), serve principally educational functions—convincing citizens of the "political and social unanimity of the Soviet people," putting forward candidates as paragons for emulation, legitimating to the population the regime's power and policy by nearly unanimous ratification of its chief power holders at all levels, and communicating the programs of the Party (Brunner 1982, 36–42; Kenez 1985, 62, 251; Speranskii 1975, 112–13).

Citizen involvement also extends the policing and monitoring (*kontrol'*) capacity of the bureaucracy. The Soviet volunteer militia (*druzhiny*), comrades' courts, people's controllers, and residence and workplace committees not only enforce the laws and maintain social control through such mechanisms as the passport regime but also, to quote Friedgut's (1979, 258, 265) description of the *druzhiny*, serve "to bring a crushing weight of conformity against those who, though not violating any law, are not acting in an approved manner" (Adams 1977).

Leninist coproduction also extends the capabilities of the bureaucracy in such realms as economic production, service delivery, and military preparedness (Lane and O'Dell 1978, 32–36). For example, Stalin's policy of rapid industrialization, according to Brown (1966, 53), made the representative function of Soviet trade unions "obsolete" and made them "face towards production." Osoaviakhim's "broad aims," according to Odom (1973, 88)—in addition to propaganda and socialization—were to train its volunteer membership in basic military skills and mobilize them for military preparedness by tapping material, financial, and labor resources for defense and production.

To minimize the opportunities for political participation that this enlistment of involvement in coproduction might create, the Leninist model of development assiduously removes most decision making from the institutions of coproduction, particularly through centralization of decision making within institutions and transfer of this function to the Party (Lee 1979, 123, 126; Urban 1982). In Soviet volunteer public organizations, as Odom (1973, 14) notes, "the political processes for selecting [the organization's aims] remain largely within the government or ruling bureaucracy, giving the ruling elite an upper hand in goal and program selection." In popular balloting for soviets—particularly for those proximate to the centers of decision making—the Party's tight control over nominations, the uncontested elections, and the apparent guarantee of majorities for all but a few candidates strips even this political activity of its participatory functions.

At the same time that it strips these participatory opportunities from political activity the Leninist model of development has tolerated a limited realm of participation as a "safety valve"—individual contacting for particularized benefits. Appeals to administrators, either directly or through the intermediary of a newspaper, soviet deputy, local Party official, or procurator are reportedly common. Noting the frequency with which émigrés report they engaged in the practice of "working the output side" while still in the Soviet Union, DiFranceisco and Gitelman (1984, 605) argue that "the way Soviet people relate to the political administrative system is to go through the motions of participation in the nominally democratic process of making decisions, but to put far more serious effort into trying to influence the way decisions are implemented." Bialer (1980, 167) suggests the significance of this behavior when he describes this "low politics" as "the very substance of the Soviet system of political participation."

Leninist Stability

This combination of enlisted involvement and tolerated particularized contacting plays a critical role in the reduction of further participatory pressures. The Leninist strategy operates on each of the factors linking social mobilization (*S*) to participation (*P*) so as to reduce such pressures: Specifically, it controls the growth of social mobilization (*S*), while expanding both economic development (*E*) and mobility opportunities (*M*).

The control over the modernization process itself inherent in the Leninist model of development expands the opportunities for leaders to prevent social mobilization from exceeding either economic development or mobility opportunities. Significant in this context—particularly in light of the contribution education has been found to make to participation in developing societies—the combination of forced-draft industrialization and limitations on the openings in higher education has meant that economic development in many years soared beyond the trained cadres available to lead society.

The combination of involvement in coproduction and particularized contacting diminishes participatory pressures by providing both economic and mobility opportunities. In the early stages of development involvement in coproduction is essential to the rapid economic growth that expands both the resources and mobility opportunities to satisfy the rising aspirations of a population undergoing rapid social mobilization. The Party makes this link between involvement and personal mobility explicit by making involvement such as the party assignment (*partiinoe poruchenie)* a prerequisite for advancement (Silver 1987, 122–25; Zimmerman 1987, 336–37).

Significantly, neither enlisted involvement nor particularized contacting appears to have an automatic spill-over effect into other forms of participation. According to Verba, Nie, and Kim (1971, 43–44, 57–59), particularized contacting in democracies "stands out from other acts [of participation] because of the narrowness of the focus of the act—it involves no necessary concern with broader social issues, but rather a concern with the narrow parochial problems of the activist." It does not appear to be part of a more general participation syndrome (Huntington and Nelson 1976, 43). Specifically, the skills that give rise to, or sustain, political participation are not required by, nor necessarily learned through, enlisted involvement or particularized contacting: enlisted activity to repair an apartment block, build a stadium, or collar a hooligan does not require—nor does it necessarily contribute to—an ability to empathize, generalize, abstract, or defer rewards. Although the rewards for involvement tend to be collective, and those for particularized contacting are usually individual, both tend to be proximate, immediate, and tangible. Moreover, neither involvement nor particularized contacting prepares citizens for the open contest of interests that constitutes politics.

The combination of involvement and particularized contacting may actually reduce the probability of other forms of participation—particularly destabilizing forms of participation. By permitting a resolution of individual grievances at the level of "low politics," particularized contacting may preclude the generalization of grievances into collective action that addresses the "high politics" of major political issues reserved to the leadership (Bialer 1980, 166). Both forms of political activity may contribute to the legitimacy of the political system by providing the tangible, immediate, and proximate rewards desired by individuals and by creating a personalistic tie between the system and the citizen. And both may in some subtle psychological way substitute for other forms of participation in the minds of many citizens (C. Black 1958, 575–76). Thus, in the short run, the Leninist strategy of involvement in coproduction and particularized contacting can relieve or attenuate the growth of participatory pressures in a developing polity.

In short, the combination of involvement and particularized contacting is well suited to a population at earlier stages of social mobilization confronting governmental agencies that provide a wide range of services. It is also well suited to the purposes of a regime that seeks to slow the growth of participation as social mobilization advances (J. Scott 1972, 145–57). It permits the Leninist regime to postpone the crisis of participation and the institutionalization of participation.

The Crisis of Leninist Involvement

Nonetheless, this Leninist developmental strategy appears to contain seeds of its own potential dysfunction. First, in the longer term, involvement in coproduction increases the social mobilization of the population and so may increase the potential for participatory pressures. Surveys of Soviet émigrés show a sharp rise in attentiveness to politics, political competence, and participation, with education. Moreover, involvement in the implementation of policy may "create expectations about wider citizen influence that conventional organizations cannot satisfy" and even train individuals in skills of collective action in a highly bureaucratized environment that might make participation possible (Bahry 1987, 76, 81, 89; DiFranceisco and Gitelman 1984, 607–9; Zimmerman 1987, 336–40).

Second, the modernization that enlisted involvement promotes tends to prevent further involvement from creating new mobility opportunities and so stemming the growth of participatory pressures; that is, involvement would appear to be most effective at promoting economic development and creating mobility opportunities in the early stages of modernization, when the coproduction tasks have a low capital-labor ratio and require fewer inputs of expert labor relative to unskilled labor. As Odom (1973, 19) notes, "It is where polities are in transition, where the political leadership is breaking down traditions, building new social patterns, . . . and in possession of relatively abundant labor forces that mass voluntary organizations seem especially appropriate." For the individual citizen, as economic growth slows and mobility opportunities no longer expand so rapidly, involvement is a less certain means of fulfilling aspirations in later stages of modernization (Bushnell 1979).

As a consequence, the longer-term result may be autonomous disinvolvement—the withdrawal of citizens from institutionalized political activity—and autonomous participation outside institutionalized channels. The less complete the initial forced departicipation of the population and the more the regime flags in its efforts to sustain enlisted involvement and forced departicipation, the sooner these dysfunctions will appear (Bahry and Silver 1987, 1085-91; Nelson 1982, 321; Pirages 1972).

Autonomous Disinvolvement and Participation

The foregoing discussion predicts a specific empirical relationship between modernization, on the one hand, and involvement and participation, on the other; that is, in a society such as the Soviet Union pursuing the Leninist developmental strategy, involvement should reach its maximum point at intermediate levels of modernization (concave, quadratic).[3] Conversely, participation should reach its lowest point at these intermediate levels of modernization (convex, quadratic).[4]

In addition, the foregoing discussion predicts that insofar as uninstitutionalized participation increases the potential for destabilizing political behavior, this behavior should decline in the initial stages of modernization but then rise in later stages (convex, quadratic). This is just the reverse of the relationship that serves as the empirical basis for Huntington's (1968, 39-45) thesis: the Leninist strategy of development, in a sense, appears to turn the Western curvilinear relationship on its head.

That the first two hypothesized relationships between modernization, on the one hand, and involvement and participation, on the other, actually describe a central development dynamic of Leninist politics is confirmed by diverse evidence. Inevitably, due to the limitations of data from the Soviet Union, this evidence is at times indirect or incomplete; and we must suspect some of this evidence for its potential for statistical bias. Nonetheless, this evidence taken together does not suffer a systematic bias that would result in a common statistical testing error across sources. And the cumulative effect of these diverse, confirmatory data suggests a common pattern.[5]

Declining Involvement

The curvilinear (concave, quadratic) relationship between modernization and involvement is manifest in recent years in declining rates of involvement in the ritualistic, propaganda and administrative activities of the regime. For example, Soviet elections—the most extensive form of involvement—showed growing turnout of the adult population over the first decades of Soviet power but, more recently, a fairly consistent decline in the proportion of adults voting (Figure 1).[6] Indeed, the decline in proportionate levels of electoral involvement among adults 18 years and older has now lasted three

Figure 1. Electoral Involvement/ Noninvolvement, USSR, 1926-85 (logarithmic scales)

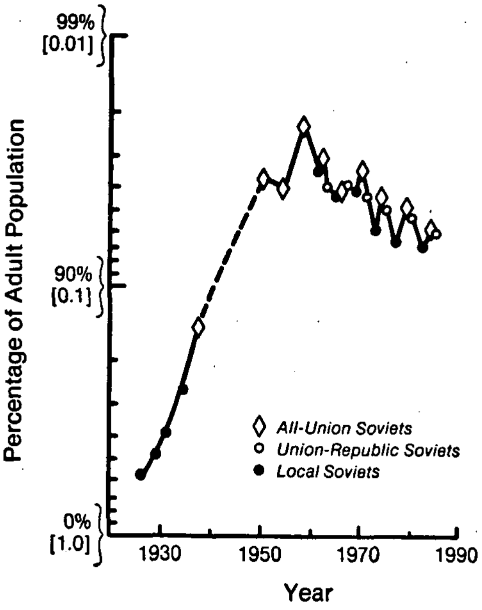
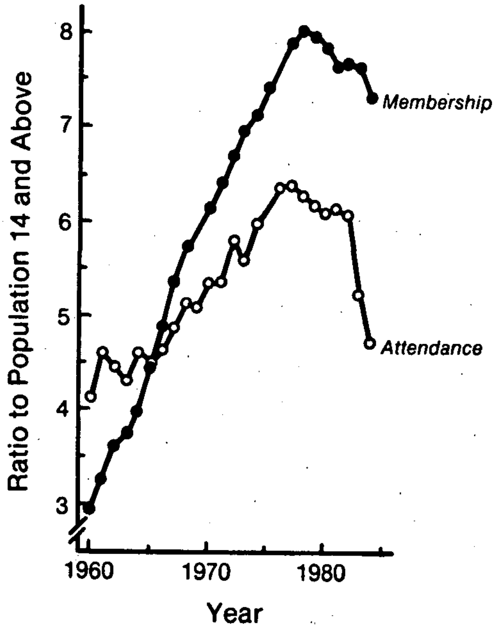


Figure 2. All-Union Knowledge Society, USSR: Membership and Attendance, 1960–84



Note: Membership = Ratio per 50,000 population; attendance = Average per population.

decades: in a simple longitudinal regression for noninvolvement all parameter estimates are significant at the .001 level (App. A, equation 1).

Still more representative of the volunteer self-administration that constitutes Leninist involvement is service on electoral commissions to register voters and administer the balloting. This unpaid activity constitutes the "accounting and control" of the electoral system. Yet the proportion of the adult population involved in this activity has fallen over the past two decades (data is only available since 1965). The parameter estimate is negative and significant at the .001 level (App. A, equation 2). From 5.95% of the adult population in the 1967 local and republic elections, this proportion fell to 4.46% in the 1985 local and republic elections.

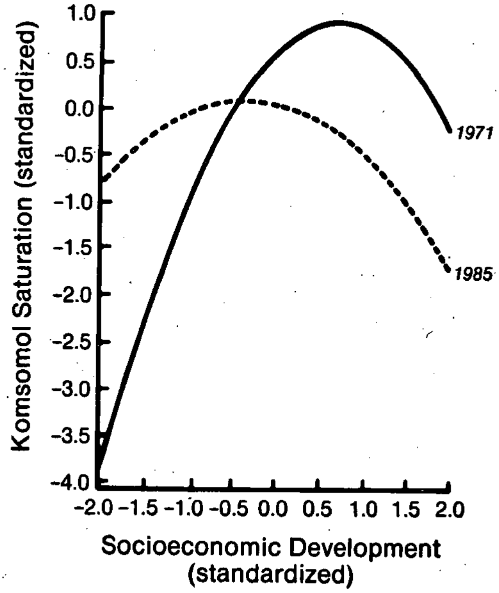
A similar decline—of more recent origin—can be seen in the pattern of involvement in the activities of Znanie, the principal voluntary, membership-based organization devoted primarily to propaganda. Founded in 1947 as the All-Union Society for the Dissemination of Political and Scientific Knowledge, Znanie boasted 38 years later a membership of 2.7 million organized in 130.8 thousand primary organizations and claimed in 1984 to have delivered 22 million lectures with an attendance of one billion. Nonetheless, the attendance rate in Znanie lectures—expressed as a ratio to the population 14 years and older—has actually fallen significantly over the past decade, losing since 1975 most of the gains made in the previous decade (Figure 2). This has been followed by a significant decline in membership rate.[7]

Still more recently, the Komsomol has experienced a reversal in its growing saturation of the young adult population, with a 10% decline in membership between 1985 and early 1988.[8] This was anticipated in cross-sectional variation of Komsomol saturation in the Soviet Union's 15 union republics: regressing Komsomol saturation in the 14–28 age cohort on socioeconomic development for 1971 yields a weak curvilinear (concave, quadratic) relationship (App. A, equation 3). The parameter estimates for socioeconomic development are individually and jointly significant at the .05 level. In 1985, these involvement rates show a still stronger curvilinear relationship, with all parameter estimates significant at the .001 level (equation 4). The curves predicted by these two equations are shown in Figure 3.[9] Not only does each curve suggest that at higher levels of development, further development brings a decline in involvement; but taken together, the different slopes of these two curves suggest that over time the relationship with socioeconomic development is becoming more negative.

Taken together, these trends in elections, volunteer self-administration, and organizational membership suggest a cumulative, aggregate process of greater disinvolvement manifest both within individual forms and in ever-more forms of involvement. Although many details of this process must await further data, it would appear that disinvolvement was first manifest in more extensive forms of activity with fewer material or career incentives attached. Indeed, in voting, which a citizen can evade (usually without retribution) by means of the certificate to vote elsewhere, net disinvolvement began in the Khrushchev years. In less extensive forms of involvement or in those with more significant "enlistment incentives," net disinvolvement began later—apparently as late as the Gorbachev years for Komsomol membership.

Underpinning these aggregate patterns are microlevel differences among individuals. The association in recent years of declining involvement with rising social mobilization (where mobility and material incentives flag) is reflected in surveys of both Soviet citizens and émigrés. Patrushev (1986, 58, 61) finds in two surveys of Soviet urban dwellers (for 1965 and 1982) "a tendency toward the individualization of spare time," which he attributes to industrialization and urbanization: the proportion of respondents engaged in social labor (*obshchestvennaia rabota)* and the average amount of time devoted to this were markedly lower in the 1982 survey than in 1965. In interviews of recent émigrés from the Soviet Union, Karklins (1986, 456, 460) reports a rate of nonvoting among respondents with at least some higher education (40%) that is nearly three-quarters higher than among respondents with no more than a secondary education (23%) (Friedgut 1979, 118; Zimmerman 1987, 336). The strong association at the microlevel between nonvoting and education in the recent period is suggested by the responses of the 2,793 émigrés in this Soviet Interview Project (App. B). The probability that a respondent failed to vote at least once (during the "last normal" five-year period before the decision to emigrate) rises significantly with educational attainment (in App. B, equation 1, *E*, holding *I* constant). But in addition to this direct effect, educational attainment *(E)* appears to increase nonvoting indirectly by contributing to the growth of attitudes such as interest in politics *(P)* and hostility toward the system *(H)*.[10]

Figure 3. Socioeconomic Development and Komsomol Involvement, USSR, 1971 and 1985



Note: Fitted cross-sectional curves are from Equations 3 and 4, Appendix A.

Of course, as suggested in the previous discussion, to the extent that organizations continue to promise mobility opportunities or particularistic, material rewards to membership they may escape this trend toward disinvolvement in later stages of modernization. Communist Party of the Soviet Union (CPSU) membership, which is oftentimes a prerequisite for success in careers, continues to include a growing proportion of the eligible adult population. If these rewards are tied to compulsory membership or intense pressures for membership, an organization stands a strong chance of escaping this trend. For example, trade union membership continues to grow. Similarly, membership in DOSAAF, which in 1985 boasted more than 107 million dues-paying members, continued to grow as a percentage of the eligible population (14 years and older). Indeed, from 1977 to 1985, this percentage grew from 40.4 to just over 50%.[11] The contrast between DOSAAF and Znanie is an instructive one: first, DOSAAF clubs and sports facilities offer significant enlistment incentives—frequently the only opportunity "to learn to drive an automobile, water ski, fly, hunt, parachute, be a ham radio operator, or to take part in almost any sport activity" (H. and W. Scott 1981, 310). But second, and more importantly, under the 1967 Law on Universal Military Service, DOSAAF is also responsible for *compulsory* preinduction training and instruction in military specialties for youths.

Participatory Pressures and Potentially Destabilizing Behavior

The association between declining involvement and mounting participatory pressures is supported by the finding in the Soviet Interview Project (SIP) that respondents who had dropped out of certain "universal" forms of involvement such as voting were much more likely to have engaged in "nonconformist" behavior: those who had failed to vote at least once during the last normal five-year period prior to the decision to emigrate were 2.2 times as likely to have attended an unofficial cultural event, 4.7 times as likely to have attended an unsanctioned discussion group, 5.9 times as likely to have distributed *samizdat* or *tamizdat* materials, and 6.8 times as likely to have participated in an open protest against some Soviet policy.

The curvilinear (convex, quadratic) relationship between modernization, on the one hand, and both participatory pressures and potentially destabilizing behavior, on the other, is supported by surveys of both émigrés and Soviet citizens. Comparison of the Harvard Project survey of émigrés who left the Soviet Union during earlier stages of modernization with the more recent SIP survey suggests a curvilinear relationship over time between disagreement with official policies and social mobilization. In both surveys, hostility to the Soviet system was strongly correlated with social group membership, but in the earlier group it declined rather than rose as one ascended the socioeconomic ladder (Inkeles and Bauer 1961, 260). In the second group, the relationship is positive: Zimmerman (1987, 346) finds social mobilization among respondents related negatively with "regime-conforming behavior" or positively with efforts to read *samizdat,* listen to foreign radio broadcasts, or avoid military service (Bahry 1987, 85; Karklins 1986, 456, 460; Silver 1987, 116–22, 132–33). These results are consistent with the evidence (App. C) that among these émigrés in the Soviet Interview Project, educational attainment (E) is directly and positively related to the probability that respondents had engaged in unsanctioned behavior such as attending an unofficial cultural event, participating in an unsanctioned study group, distributing *samizdat* or *tamizdat,* and participating in a protest against some Soviet policy. Moreover, educational attainment contributes indirectly to this probability by increasing interest in politics (P) and hostility toward the regime (H). In the contrasting results of these two surveys one sees reflected a curvilinear relationship in which social mobilization initially brought declining, but in later years then brought rising dissidence.

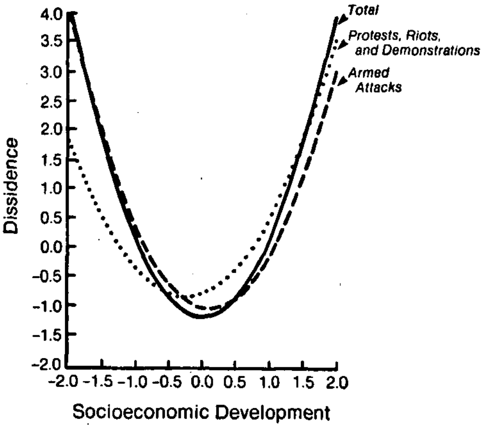
This is consistent with the conclusion—reported in a number of Soviet surveys—that dissidence (as measured by disagreement with official policy) shows a curvilinear relationship with educational attainment and is particularly high among the most educated. *Izvestiia*'s 1966 survey of 26 thousand readers found a curvilinear relationship between education and disagreement with official interpretations of events reported in the paper. Disagreement was highest at the extremes of education—those with only a primary education and those with a higher education (Davydchenko and Shliapentokh 1968, 23–29; Mickiewicz 1981, 133–37). Firsov's (1967, 42–45) survey of 1,916 television viewers in Leningrad found a similar curvilinear relationship between education and dissatisfaction with news and public affairs programming. Shliapentokh (1975, 99) has summarized surveys of Soviet readers with the observation that criticism of the informational content of official publications is highest among respondents with the highest levels of educational attainment (Hollander 1972, 67, 69, 120). *Trud*'s survey of readers found that highly educated readers were more critical of the newspaper's content than average workers (Skvortsov 1968, 46–48). In *Izvestiia*'s 1966 survey of 26 thousand readers, only 11.9% of the respondents reported they had disagreed with official interpretations of events reported in the paper, but in a preliminary survey of experts, 49% of the "representatives of exact and natural sciences" felt that average readers disagree with the governmental paper (Davydchenko and Shliapentokh 1968, 23–24). And criticism of the Party's political instruction programs is also reportedly higher among the most educated (Kniazeva 1974, 66).

Other Leninist Polities

Insofar as the crisis of postponed participation is not simply a product of Soviet idiosyncracies but of the Leninist development strategy, the patterns noted above between social mobilization on the one hand and involvement, participation, and potentially destabilizing behavior on the other should be manifest in other societies that have followed the Soviet Union in this strategy.[12] In China, for example, Townsend (1969, 3–6, 191) notes at the end of two decades of Communist rule a "relative weakness of institutionalization," yet argues that this "has not led to political instability and decay" (Cohn 1979; Fairbank 1983, 367; Falkenheim 1978). Commenting on the recent turmoil in Eastern Europe, Korbonski (1983, 212–13) argues that "there is little doubt that the crisis of participation in the area has been largely due to the emergence over the past several years of the classic Huntingtonian syndrome of mass mobilization unaccompanied by institutionalization" (Gitelman 1977; LeoGrande 1978; Schulz 1981; Simon 1983).

Although consistent cross-national measures of involvement and participation in Leninist polities appear to be unavailable, events data provide a more direct test of the relationship between modernization and potentially destabilizing behavior than is possible within the Soviet Union itself. Cross-national events data—although inevitably incomplete in Leninist societies—should not show a systematic measurement bias within the group of Leninist polities and so should permit comparison of the levels of potentially destabilizing behavior. These, in fact, show a strong curvilinear (convex, quadratic) relationship with socioeconomic development: the highest rates of dissident events are found in the least-developed and most-developed Leninist polities, the lowest rates in polities at intermediate levels of development. Regressing dissidence as measured by the number of protests, riots, and armed attacks per million population between 1978 and 1982 on the level of socioeconmic development yields a parameter estimate that is significant at the .001 level

Figure 4. Protests, Riots, Demonstrations, and Armed Attacks per Capita, Leninist Polities, 1978–82



Note: Fitted curves are from equations, Appendix D (standardized).

(App. D, equation 1) (Taylor 1985, pt. 3). Dividing these events of dissidence into demonstrations and riots, on the one hand, and armed attacks, on the other, shows that the latter is more typical of the less-developed Leninist societies and the former of the more-developed, but that both conform to the curvilinear relationship found for all dissidence. These relationships are shown in Figure 4.

The curvilinear relationship between dissidence and development should be manifest longitudinally in declining dissidence among the less-developed Leninist societies and growing dissidence among the more-developed. Indeed, societies ruled by Leninist regimes throughout the 23-year period from 1960 to 1982 exhibited two diametrically opposite trends in the level of dissidence: the eight more-developed societies (the Soviet Union and East European states, except Albania) experienced a significant secular growth in dissidence over those 23 years. The time series regression of total dissidence in these societies yields a positive parameter estimate that is significant at the .01 level (App. D, equation 4). The five less-developed societies (Albania, China, Cuba, Mongolia, and North Korea) experienced a significant secular decline in dissidence over that same period. The parameter estimate is negative and significant at the .001 level.[13]

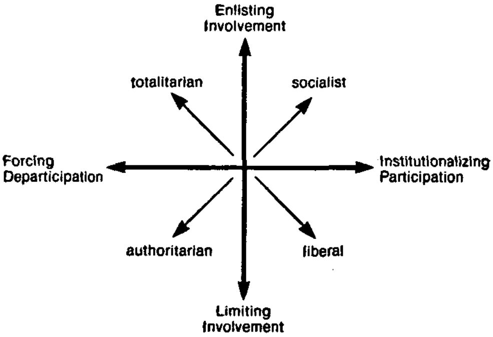
In sum, a significant body of evidence—remarkably diverse for Leninist societies—appears to confirm a long-term curvilinear relationship of social mobilization with disinvolvement, participation, and potentially destabilizing behavior. The available evidence seems to confirm this not only at the macro level with aggregate data, but also at the micro level with individual data. The former evidence is both cross-sectional and longitudinal.

Policy Responses to the Leninist Crisis of Participation

Gaps between institutions and participation creates points in the development of a Leninist regime at which leaders must seek to preclude decay. The crisis of participation in recent years has not gone unnoticed among Soviet leaders. Gorbachev warned the January 1987 Central Committee Plenum of "a decline in interest in public affairs."[14] At the 19th Party Conference in June 1988, he frankly stated that "the price paid for [the command-administrative methods] has proven to be harsh—indifference, slackening in social activism of the masses, alienation of the working people from public ownership and management."[15] Of course, although presently more acute, this crisis did not originate under Gorbachev but has been recurring and deepening since the Khrushchev years.

The key to whether this threat grows and is manifest in actual instability would appear to be the policy responses of these leaders. As suggested by Huntington's analysis, the gap between institutions and participation must be closed with either renewed forced departicipation or institutionalization of participation. In the Leninist strategy, however, this is naturally tied to policy choices to enlist or limit involvement. Seen as two dichotomous choices, these describe four distinctive policy strategies (Figure 5): (1) departicipation with efforts to enlist involvement would represent movement toward the mobilized society of *totalitarian* theory; (2) departicipation with abandonment of efforts to enlist involvement would represent a strategy more typical of *authoritarian* regimes; (3) efforts to institutionalize expansion of participation without enlisting involvement would represent first steps toward a *liberal* society and, (4) efforts to institutionalize expanding participation and enlist involvement in coproduction would constitute a new synthesis that could move the Soviet Union toward the Leninist ideal of *socialist* society—a society that brings citizens into political life both as participants and in the everyday administration of affairs.

Figure 5. Alternative Leninist Reform Strategies



Note: These strategies are represented as movement from the status quo. The status quo at any point in time is represented by the origin. The label for each strategy represents the direction of policy or its end point if pursued to fulfillment. (In a static comparative analysis this can be transformed into an *n* × *n* distinction among polities.)

Previous reforms in the post-Stalin period in response to the crisis of postponed participation have frequently been characterized as representing different balances between elitism and populism (Breslauer 1982, Hough 1976). But an equally important distinction—one less frequently made—is their differing combinations of participation and involvement in coproduction. Khrushchev's populism—as it is often called—meant expanded popular involvement in coproduction through growth of nonstaff (volunteer) administration, transfer of administrative functions to public organizations, and reduction of the apparatus. Yet, Khrushchev's populism was actually far closer to Stalin's policies as it bore on participation in decision making. The expansion of popular participation was extremely limited and indirect—effected through greater decentralization of decision making to levels of the Soviet system where popular inputs were more likely to be taken into account; devolution of administrative responsibilities upon soviets and public organizations, with attendant possibilities for popular influence over administration; and modest expansion of the electivity and accountability of those bodies. Indeed, Khrushchev's policies gave special emphasis to the acute problem of preventing the involvement championed from exceeding acceptable bounds and becoming participation. His policies used the instrumentalities of involvement themselves (e.g., popular militia, comrades' courts) to check the expanding popular involvement with heightened emphasis on social control, discipline, and homogeneity in the population. At the same time Khrushchev sought to contain elite participation by organizational and popular assaults on the bureaucratic prerogatives of state officials, expanded criticism, "status equalization between the apparatchiki and the masses," and an "adversarial relationship between state bureaucrats and their clients" (Breslauer 1982, 77, 103). In short, by seeking to check the influence of potential participants who were most likely to take a real share of decision making power, Khrushchev's so-called populism used its expansion of popular involvement in coproduction to limit participatory pressures (here, a more *totalitarian* strategy).

Brezhnev's elitism—as it is often labeled—in fact represented a more significant break with the previous balance of enlisted involvement in coproduction and forced departicipation (here, a more *liberal* strategy). Brezhnev's policies limited involvement in coproduction somewhat by reducing the intrusion of volunteers in the implementation of policies (e.g., nonstaff departments), dismantling the Party-State Control Commission, and curbing the devolution of administrative responsibilities on to public organizations. But Brezhnevian practice also brought a real expansion of participation. As popular involvement in the implementation of policies was limited, elite participation was coopted into the decision making process. Breslauer (1982, 193–94) notes that in Brezhnev's early speeches as general secretary, "specialists were accorded extensive recognition and praise . . . , and were assured that their advice would be heeded by decision-makers." And compared to the limited and indirect expansion of popular participation under Khrushchev, the expansion of elite participation in decision making under Brezhnev was real and direct. As Cocks (1976, 158–59) has argued, the "deconcentration of power," which was particularly strong under Brezhnev, led to "the devolution of authority to the major institutional centers and bureaucratic subsystems in Soviet society"; and with this, "the various strategic elites [were] given a greater voice in policy making" (Bialer 1976, 32; Hammer 1974, 319–20; Hough 1973, 62–63). Nonetheless, elements of forced departicipation that continued under Brezhnev's policy attempted to prevent this expansion of elite participation from exceeding acceptable bounds: Brezhnev checked expanding elite participation with new pressures against dissidents within the elite, including campaigns for "iron discipline" within the Party, intensified political indoctrination and Party *kontrol'* within the scientific community, and heightened vigilance against dissidence within the cultural intelligentsia.

The strategy articulated by Gorbachev represents a new synthesis (a *socialist* strategy). The practical proposals he has put forward suggest a willingness to innovate with limited expansion of both popular participation and involvement, while maintaining elite expert participation. Like Khrushchev, Gorbachev continues to link the need "to deepen socialist democracy" to the problem of enlisting involvement in coproduction. Indeed, in his report to the 19th Party Conference he names, as the first basic task of political restructuring, "to do everything to see to it that millions and millions of working people are included in the administration of the country."[16] And he underscores that democratization is essential to mobilizing the energies of the working people to implement the policies of *perestroika*.[17] Yet in his discussions of "deepening democracy," Gorbachev appears to have added to this theme a meaning not employed by his predecessors by suggesting that genuine forms of popular participation might be cultivated. Gorbachev has suggested that popular political activity should be an input to policy making by supporting change and standing as an obstacle to later regression ("a guarantee of the irreversibility of restructuring") against the forces of conservatism.[18] And he has even accorded dignity to particularistic social interests by calling for "a mechanism for the unhindered formation and identification of the interests and will of all classes and social groups and their coordination and realization in the domestic and foreign policies of the Soviet state." For example, in the context of his discussion of the unresolved "women's question," Gorbachev argues that one "reason why this situation would last for years was the fact that women's opinions were not carefully considered. They do not have proper representation in executive bodies."[19]

A closer look at his reports to the 27th CPSU Congress and 19th Party Conference reveals that Gorbachev's specific proposals to achieve further "democratization" balance six approaches to expand both involvement and participation: (1) increased power for soviets at all levels relative to their respective executive agencies by expanding their powers, elevating the status of deputies, and improving their procedures, including their procedures for elections of executive and administrative officers; (2) further decentralization of decision making to local soviets by transferring "many powers to the local level," expanding their authority over local economic entities, and granting them some independent fiscal authority; (3) expanded "direct democracy" not only to communicate popular opinions to decision makers but also at the local level to vote "much more often than is now done, especially on questions that vitally affect people and on which there is no single opinion"; (4) greater popular *kontrol'* or supervision over administrators; (5) strengthened powers and greater autonomy for public organizations vis-à-vis soviets and state administrations, including transference of some state functions to public organization and direct representation of public organizations in a new all-union congress of people's deputies; and (6) improved mechanisms of nomination and election for selecting deputies to soviets and the officials of public organizations.

Like Brezhnev, Gorbachev defends the social status and prestige of experts (including not only natural scientists but even social scientists!) and cautions against a "disdainful attitude" toward administrators, but Gorbachev's proposals contrast sharply with those of Brezhnev in that they go further in their recommendations for popular participation. Brezhnev's proposals concerning popular participation had stopped at solutions that merely shift powers among state institutions (e.g., decentralization; transfer of administrative functions from executive committees to soviets). Gorbachev's go on to suggest expanding the authority of the citizenry over the soviets either directly through improved mechanisms of popular election or indirectly through the combination of extending popular elections to public organizations and expanding public organization input to soviets. Gorbachev's proposals even suggest indirect popular control over administration through the combination of improved popular elections of soviets and soviet elections of administrators (Hahn 1987, 29–39; Hill 1976, 481–95).

Although Gorbachev's proposals share many points in common with the "populism" of Khrushchev, they differ in their balance between involvement and participation: they place somewhat less emphasis on transfer of functions from administrative agencies to soviets and from state agencies to public organizations. And they are significantly more circumscribed in prescribing direct popular involvement in administrative agencies such as nonstaff departments. They give greater emphasis to perfecting the formal mechanisms of accountability and *kontrol'* such as elections and revived Party-state control committees. And they carefully balance the demands of "a real expansion of working people's participation in administration" against the demands for greater professionalism in administration.

As modest as Gorbachev's proposals, and as problematic as their success may be, they represent a new synthesis of participation and involvement; they envision at least modest institutionalization of expanded participation by both citizens and interest elites; and they give greater emphasis to infusing some real substance into the formal mechanisms of democratic control. The success of the Gorbachev proposals may depend on whether the Leninist institutions of involvement are themselves adaptable enough to accommodate participation (Hahn 1987, 29–39; Jowitt 1975, 72; Pravda 1978, 192–93). If implemented, they might inaugurate a transition of institutions of involvement in coproduction to institutions of both involvement and participation.

Implications

The Soviet Union achieved its stability in the early stages of development not by institutionalizing participation but by forcing departicipation and substituting a functionally distinct form of political activity—involvement in coproduction. In the early stages of development this enlisted involvement brought the rapid economic growth and expanding mobility opportunities that satisfied the rising aspirations of a population undergoing social mobilization. Involvement was a path to satisfying individual expectations both because it contributed to the growth of resources and opportunities to satisfy aspirations and because the Party set it as a requisite for advancement. Involvement was also the essential complement to forced departicipation. In later stages of modernization, however, as economic growth slowed and mobility opportunities no longer expanded so rapidly, involvement has become a less certain means to fulfilling aspirations. At this point, participatory pressures have begun to rise. If the regime flags in its coercive pressures to promote involvement and suppress participation, it may face a mounting crisis of spontaneous withdrawal from its institutions of involvement and rising participation outside institutionalized channels.

This rising disinvolvement and these participatory pressures need not, of course, produce instability. Yet they may well betoken a growing threat to stability. As Bialer (1980, 130, 139, 158) argues, "compliance patterns and levels" in the Soviet polity are central to its stability, and "the rise of nonmaterial expectations would present dangers to the system's stability." The rise of disinvolvement, extra-institutional participation, and potentially destabilizing behaviors are important as symptoms of potential instability because they indicate an erosion of compliance patterns in a Leninist society and growing expectations of institutionalized access to political decision making.

In short, although many have accepted Huntington's (1968, 43) conclusion that "modernity means stability and modernization instability," the Leninist strategy of development appears to turn this on its head: modernization means stability, modernity instability. Of course, the relationship is not as deterministic as this, and depends on the policy choices made by leaders.

The foregoing analysis of the Leninist developmental strategy diverges throughout from Huntington's not only in the specifics of applying his institutionalization thesis but, more fundamentally, in its very approach to the political consequences of modernization under Leninist regimes. Huntington and Moore (1970, 511) have argued, "the political evolution of a revolutionary one-party system . . . is less affected by the process of modernization since the most important political consequences of that process have already been brought to a culmination in the revolutionary upheaval that brought the system into being.[11] Yet if the Leninist polity typically does not at first institutionalize this culmination of "the most important political consequences" of the modernization process, it cannot—even within the confines of Huntington's analysis—possibly escape the consequences of the modernization process for their further political evolution.

While positing the importance to political stability of a balance between participation and institutionalization, the foregoing also diverges from Huntington's analysis in the importance it assigns the institutionalization of participation to achieve this balance in the early stages of modernization. The Leninist strategy of development has shown that the participation-institutionalization gap that frequently attends early modernization may also be closed by alternative means, such as forced departicipation in conjunction with enlisted involvement in coproduction.

Even as it points to the limits of the Leninist regime's autonomy from the developmental process, the foregoing analysis underscores the policy impact that the Leninist regime can have on that process. And so, by pointing up the ability of Leninist regimes to modify—or at least delay—the political consequences of the modernization process, this analysis is of a piece with the recent realization that we must "bring the state back in" or "take the state seriously." Specifically, the Leninist strategy of development represents not only a different response to a similar developmental process but also a means to modify at least the sequences of that process.

The ability to postpone the crisis of participation—even if not indefinitely—may be a very important asset to Leninist regimes. This may permit these regimes to address the multiple crises of development seriatim rather than being overwhelmed by cumulative penetration, distribution, identity, and participation crises. If, as Rustow (1967, 123–32) argues, the most stable developmental sequence permits the regime to postpone the expansion of participation until after the establishment of unity and authority, this ability to postpone the crisis of participation is particularly important. For as Weiner (1971, 159) warned, "One of the most disruptive crises in modern times has been caused by the efforts of those not in power to gain access to power or to influence decisions made by governing elites."

It is tempting—particularly in the current optimism inspired by *perestroika*—to see in this developmental sequence a parallel with polyarchical development in the West. In the slow expansion of elite participation and the growing legitimation of elite policy debate under Brezhnev and now *glasnost'*, one might find the first tentative steps toward contestation and what Dahl (1971, 7–9) has labeled a competitive oligarchy. In the subsequent cautious expansion of participation one might find the beginnings of popularization to provide a second ingredient of democratization. The similarities, which are at least metaphoric, may instruct us to look for indications of growth in this direction and may offer benchmarks for comparing the Leninist developmental strategy with its alternatives. Yet to the extent that Gorbachev succeeds in carrying out his new synthesis of involvement and participation, he may move the Soviet Union along a path quite different from that in liberal polities.

Appendix A: Political Involvement, Soviet Union (OLS Results)

1. *Electoral Noninvolvement, 1955–85* (longitudinal)

$$E = 2.36 + .14T + 1.00L$$
$$t = (10.11) \quad (10.26) \quad (4.45)$$
$$F = 60.84,\ N = 19,\ R^2 = .88$$

where

E = percentage of population 18 and older not voting
T = time
L = local election only

2. *Involvement in Electoral Commissions, 1965–85* (longitudinal)

$$\begin{array}{llll} C & = 609.31 & - & 8.12T \\ t & = (68.47) & & (-11.09) \end{array}$$

$F = 122.90$, $N = 10$, $R^2 = .94$

where

C = proportion (per 10,000) of population 18 and older involved
T = time (1965 = 1)

3. *Komsomol Saturation Rate, 1971* (cross-sectional)

$$\begin{array}{llllll} K & = 46.09 & + & 2.86S & - & 3.22S^2 \\ t & = (27.36) & & (1.83) & & (-2.21) \end{array}$$

$F = 2.75$, $N = 15$, $R^2 = .31$

where

K = percentage of 14–28 age cohort in the Komsomol
S = socioeconomic development (mean of standardized measures of urbanization and trade turnover)

4. *Komsomol Saturation Rate, 1985* (cross-sectional)

$$\begin{array}{llllll} K & = 61.32 & + & 2.71S & - & 1.21S^2 \\ t & = (42.63) & & (4.77) & & (-4.38) \end{array}$$

$F = 16.29$, $N = 15$, $R^2 = .75$

where

K = percentage of 14–28 age cohort in the Komsomol
S = socioeconomic development (as in equation 3)

Appendix B: Nonvoting in the Soviet Union (Logit Results)

1. *Nonvoting (N)*

$$\begin{array}{llllll} N & = 411.01 & + & 7.32E & + & .05I \\ se & = (13.41)^* & & (1.31)^* & & (.02)^* \\ & - 1.17A & + & 32.77P & + & 23.46H \\ & \;\;(.18)^* & & (4.87)^* & & (8.74)^* \\ & + 10.67D & & & & \\ & \;\;(1.72)^* & & & & \end{array}$$

Number of cases = 2,665

2. *Interest in Politics (I)*

$$\begin{array}{llllll} P & = 332.84 & + & 14.01E & + & .10I \\ se & = (13.11)^* & & (1.34)^* & & (.03)^* \\ & + .31A & & & & \\ & \;\;(.18) & & & & \end{array}$$

Number of cases = 2,669

3. *Hostility toward Soviet System (H)*

$$\begin{array}{llllll} H & = 326.68 & + & 4.94E & - & .08I \\ se & = (22.59)^* & & (2.22)^* & & (.06) \\ & + .44A & & & & \\ & \;\;(.32) & & & & \end{array}$$

Number of cases = 2,669

*Coefficient estimate is at least twice the magnitude of the standard error (in parentheses). Note that the logit model is log $(p/1 - p)/2 + 5)$. For presentation, all coefficients and standard errors are multiplied by 100.

where

N = Respondent sometimes did not vote (or never voted) during last normal period (VOTE: dichotomous)
E = Respondent's educational attainment (REDHIQ: ordered)
I = Respondent's gross monthly salary at end of last normal period (GROSLST)
A = Respondent's age in 1979 (79-YRBORNQ)
D = Number of policies on which respondent disagreed with regime's policies by preferring privatization of medical care, heavy industry, or agriculture, by supporting right to strike, and by opposing residence permit system
P = Respondent's claim to be very interested in politics during

last normal period (POLITICS: dichotomous)

H = Respondent would "keep nothing" in present Soviet system (KEEP1: dichotomous)

Appendix C: Nonconformist Political Activity, Soviet Union (Logit Results)

1. *Attendance at Unofficial Art Show (U)*

$$\begin{array}{lllll} U = & 357.23 & + 13.02E & + .04I \\ se = & (16.37)^* & (1.71)^* & (.03) \\ & - 1.21A & + 36.79P & + 22.32H \\ & (.22)^* & (5.40)^* & (9.58)^* \\ & + 6.46D & & \\ & (1.99)^* & & \end{array}$$

Number of cases = 2,667

2. *Participation in Unsanctioned Study Group (G)*

$$\begin{array}{llll} G = & 303.01 & + 16.88E & - .01I \\ se = & (27.42)^* & (3.09)^* & (.05) \\ & - 2.05A & + 47.20P & + 15.35H \\ & (.37)^* & (4.87)^* & (13.63) \\ & + 6.94D & & \\ & (3.04)^* & & \end{array}$$

Number of cases = 2,667

3. *Distribution of Samizdat/Tamizdat (S)*

$$\begin{array}{llll} S = & 168.42 & + 22.85E & + .03I \\ se = & (39.62)^* & (4.52)^* & (.05) \\ & - 1.53A & + 95.19P & + 7.16H \\ & (.46)^* & (10.95)^* & (18.13) \\ & + 10.56D & & \\ & (3.91)^* & & \end{array}$$

Number of cases = 2,665

4. *Participation in Protest against Soviet Policy (R)*

$$\begin{array}{llll} R = & 256.82 & + 6.87E & - .20I \\ se = & (60.80)^* & (6.43) & (.14) \\ & - 1.66A & + 86.37P & + 29.17H \\ & (.83)^* & (19.81)^* & (25.54) \\ & + 9.91D & & \\ & (7.15) & & \end{array}$$

Number of cases = 2,663

*Coefficient estimate is at least twice the magnitude of the standard error (in parentheses). Note that the logit model is $\log[(P/1 - p)/2 + 5]$. For presentation, all coefficients and standard errors are multiplied by 100.

where

U = attendance at an unofficial art show, poetry reading, or concert during last normal period (GOART: dichotomous)

G = participation in unsanctioned study or discussion group during last normal period (GOSTUDY: dichotomous)

S = participation in distribution of *samizdat* or *tamizdat* during last normal period (GIVESAMZ: dichotomous)

R = participation in open protest against some Soviet policy during last normal period (PROTEST: dichotomous)

all independent variables are as defined in Appendix B

Appendix D: Political Dissidence, Leninist Regimes (OLS Results)

1. *Total Political Dissidence, 1978–82* (cross-sectional)

$$\begin{array}{lll} D = & -533.49 & + 928.06S^2 \\ t = & (-3.00) & (5.63) \end{array}$$

$F = 31.75$, $N = 23$, $R^2 = .60$

where

D = dissidence (sum of protests, riots, and armed attacks per million population, 1978–82)

S = socioeconomic development

(mean of standardized measures of per capita caloric intake, infant mortality, and life expectancy, 1980)

2. *Demonstrations and Riots, 1978–82* (cross-sectional)

$$\begin{array}{lcccc} R & = & -52.31 & + \; 80.81S & + \; 157.56S^2 \\ t & = & (-.78) & (2.04) & (2.46) \end{array}$$

$F = 3.66$, $N = 23$, $R^2 = .27$

where

R = demonstrations and riots per million population, 1978–82
S = socioeconomic development (as in equation 5)

3. *Armed Attacks, 1978–82* (cross-sectional)

$$\begin{array}{lccc} A & = & -443.85 & - \; 142.56S \\ t & = & (-2.61) & (-1.42) \\ & & + \; 729.19S^2 & \\ & & (4.51) & \end{array}$$

$F = 16.69$, $N = 23$, $R^2 = .63$

where

A = armed attacks per million population, 1978–82
S = socioeconomic development (as in equation 5)

4. *Total Political Dissidence, 1960–82* (stratified longitudinal)

More developed

$$\begin{array}{lcccc} D & = & 14.09 & + \; 2.30T & + \; 141.52C \\ t & = & (1.12) & (2.57) & (6.70) \end{array}$$

$F = 24.05$, $N = 23$, $R^2 = .71$

Less developed

$$\begin{array}{lcccc} D & = & 95.45 & - \; 5.21T & + \; 453.20C \\ t & = & (5.00) & (-3.80) & (10.16) \end{array}$$

$F = 64.86$, $N = 23$, $R^2 = .87$

where

D = dissidence per million population (as in equation 5)
T = time (1960 = 1)
C = control for outliers

Notes

I wish to thank Anthony Brunello, Daniel N. Nelson, Bruce Perlman, and Debra C. Rosenthal for their invaluable comments.

1. Permitted or forced emigration is an alternative means to decimate the participant population, since the émigrés from newly founded Leninist regimes tend to come disproportionately from the upper reaches of the old order (J. Black et al. 1976, 61–66; Keefe et al. 1982, 57–58).

2. Extreme forms of demobilization include the Chinese *hsia-fang* (downward transfer) and the Kampuchean ruralization under Pol Pot.

3. E.g., Figure 3.

4. E.g., Figure 4.

5. To minimize systematic bias in the results, two precautions were taken: (1) in the selection of evidence for aggregate trends, my strategy is to attempt to be exhaustive, that is, the involvement rates presented below—to my best knowledge—are a complete set for which longitudinal and cross-sectional data are available; (2) in all instances not the *magnitude* of an activity but its *pattern of variation* (either longitudinal or cross-sectional) is examined.

6. Data on turnout in elections are from Spoktorov (1938, 26) and from *Pravda*, 15 March 1950, 18 March 1954, 18 March 1958, 6 and 19 March 1959, 21 March 1962, 7 March 1963, 28 March 1965, 14 June 1966, 26 March 1967, 22 March 1969, 17 June 1970, 20 June 1971, 23 June 1973, 19 June 1974, 21 June 1975, 25 June 1977, 7 March 1979, 1 March 1980, 26 June 1982, 7 March 1984, and 2 March 1985. Proportions are based on interpolations (Beers "ordinary" formula) of age cohort data in Mickiewicz 1973 (p. 52) and United Nations 1982 (p. 437). Omitted is the 1946 election, for which reliable population-cohort estimates are difficult.

7. Data drawn from annual editions of *Ezhegodnik Bol'shoi Sovetskoi Entsiklopedii* (1960–85); age cohorts interpolated from United Nations 1982 (p. 437).

8. Bill Keller, "Soviet Youth Arm Seeks to Rein in Political Groups," *New York Times*, 7 February 1988.

9. Data drawn from *Ezhegodnik Bol'shoi Sovet-*

skoi Entsiklopedii* (1970, 1985); age cohorts interpolated from USSR, Tsentral'noe Statisticheskoe Upravlenie 1972 and U.S. Bureau of the Census 1979. Socioeconomic data from USSR, Tsentral'noe Statisticheskoe Upravlenie 1971 (pp. 9–10, 579) and 1982 (pp. 12-13, 469). The 1985 model must be interpreted with caution, for the census bureau population estimates are projections from age cohort data in the 1970 census.

10. Moreover, the positive relationship between disinvolvement and social mobilization appears to be increasing over time: this is consistent with (although not directly tested by) the finding in equation 1 that at all levels of educational attainment, older respondents (*A*) were less likely to abstain from voting. The émigŕe interview data utilized in this article were collected and made available by the Soviet Interview Project, University of Illinois.

11. Membership data are from annual editions of *Ezhegodnik Bol'shoi Sovetskoi Entsiklopedii* (1960–85); population interpolations are from United Nations 1982 (p. 437).

12. In the empirical tests below the list of Leninist states is Holmes's (1986) list of the communist world identified by three characteristics—"a commitment to the building of socialism and eventually communism; an ideology called Marxism-Leninism; and a political system constructed on the principle of 'democratic centralism' " (see Fukuyama 1985). These states are Afghanistan, Albania, Angola, Benin, Bulgaria, Cambodia, China, Congo, Cuba, Czechoslovakia, Ethiopia, German Democratic Republic, Hungary, Democratic Peoples Republic of Korea, Laos, Mongolia, Mozambique, Poland, Romania, USSR, Vietnam, People's Democratic Republic of Yemen, and Yugoslavia.

13. In both time series equations it is necessary to control for episodic explosions of dissident activity that represent short-term outliers in individual societies. Since the purpose here is not to predict these episodes but to explain the general secular trend in dissidence, each equation includes a dichotomous term (*C*) for these years (1968–69 in the first equation, 1967 in the second) to "control for" these outliers.

14. Mikhail S. Gorbachev, "O perestroike i kadvovoi politike partii" [On restructuring and the party's cadre policy], *Pravda,* 28 January 1987.

15. Mikhail S. Gorbachev, "O khode realizatsii reshenii XXVII' s"ezda KPSS i zadachakh po uglubleniiu perestroiki" [On the progress of the implementation of the decisions of the 27th CPSU Congress and the tasks of deepening restructuring], *Pravda,* 29 June 1988.

16. See n. 15.

17. See n. 15; and Mikhail S. Gorbachev, "Molodezh—tvorcheskaia sila revoliutsionnogo obnovleniia" [Youth—creative force of revolutionary renewal], *Pravda,* 17 April 1987.

18. Mikhail S. Gorbachev, "Rech' M. S. Gorbacheva na vstreche s partiinym, sovetskim, i khoziaistvennym aktivom Latviiskoi SSR" [Speech by M. S. Gorbachev at a meeting with the party soviet and economic aktiv of the Latvian SSR], *Pravda,* 20 February 1987.

19. See n. 15.

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PERSONAL INFLUENCE, COLLECTIVE RATIONALITY, AND MASS POLITICAL ACTION

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We propose two models to explain why individuals participate in collective political action—a personal influence model and a collective rationality model. Each model overcomes the free-rider problem posed by conventional rational choice theory and left unresolved in previous research. The models are tested for legal and illegal protest behaviors, using data from a national sample and two samples of protest-prone communities in the Federal Republic of Germany. The personal influence model is supported for both forms of participation, while the collective rationality model is supported for legal protest. We discuss implications of the results for grievance and rational choice theories of collective political action.

Relative deprivation, dissatisfaction with government policies, and alienation from the political system have long been considered principal psychological determinants of individual participation in protest and unconventional political behavior (see Barnes and Kaase 1979; Gurr 1970; Muller 1972, 1979). While there have been frequent debates over the forms of discontent that are most important for particular kinds of protest in particular settings, consensus on the general applicability of the grievance theory of protest remains. As Muller and Jukam (1983, 159) explain, "People who take part in acts of civil disobedience or political violence are discontented about *something*. That is a truism" (emphasis added).

This "truism" has been seriously challenged in recent years by the rational choice approach, which, following Olson's *The Logic of Collective Action* (1965) and Tullock's "The Paradox of Revolution" (1971), argues that grievances are essentially irrelevant to a self-interested individual's decision regarding participation in collective political action such as protest or rebellion. Grievances typically represent desires for outcomes such as a reduction in inequality or a change in government policy that satisfy the definition of a public good.[1] In large groups, such as those involved in collective political protest, the contribution to the action of each ordinary member (i.e., one who is not a leader of the group) has no discernible impact on the group's overall success; therefore, the rational individual will not absorb the costs of participation (such as time, financial resources, or the threat of physical injury), since he or she will enjoy the public good in any case if others provide it.

The inability to deal with, or even to acknowledge, the free-rider problem is a major flaw of grievance explanations of political protest,[2] which assert that participation in collective political action is simply a monotonic function of the intensity of demand for the public good. However, in assuming that demand for the public good has *no* influence on behavior, the explanatory power of conventional rational choice theory either is limited because it predicts excessive abstention or else reduces participants in collective action to mercenaries whose behavior is motivated solely by private payoffs completely unrelated to the issues, goals, or grievances of social movements.

The theoretical problem is how to incorporate demand for the public good into an individual's utility calculus without violating the logic of free-riding. First, some individuals may believe that they are personally efficacious and that their participation consequently will, in fact, help contribute to the provision of the public good. When such a feeling of personal efficacy is coupled with a perception that the group is likely to succeed, public goods preferences then could interact with individual and group efficacy to produce positive expected utility from participation. Second, public goods preferences could produce expected benefits for individuals through interaction with beliefs that promote what we call "collective rationality." Individuals may realize that if everyone acted according to the logic of free-riding, the result would be a collectively irrational outcome. Therefore, they may adhere to the strategically "useful fiction" that unity is necessary for group success, that is, that public goods can be provided only through contributions from *all* group members. Alternatively, individuals may believe in an ethical norm of duty to participate in the provision of public goods that they strongly desire and may act as "calculating Kantians," willing to do their duty if enough others are doing the same. Belief in either a strategic unity principle or in a moral duty to participate may lead individuals to calculate their expected benefits based on the likelihood of *group* success. Public goods preferences, then, can be a relevant incentive to participate in collective action for those who would not free-ride because they believe that their own actions are efficacious or for those who reject free-riding because they believe in principles that encourage collective, as opposed to individual, rationality.

Public Goods and Collective Action: Alternative Models

What may be termed the simple grievance model of political protest can be expressed in an equation with public goods preferences as the primary explanatory variable as

$$E(A) = V,$$

where $E(A)$ is the expected value of participation in collective political action (A), and V is the value the individual attaches to the public good. Previous work identifies relevant public goods for protest behavior as either dissatisfaction with government policy (Barnes and Kaase 1979; Opp 1986; Walsh and Warland 1983) or more general alienation from the political system (Muller 1979; Muller and Jukam 1983; Muller and Opp 1986). We will focus on general policy dissatisfaction. The most important feature of this model is its treatment of public goods preferences as an autonomous cause of protest, even with the addition of numerous other demographic or social-psychological independent variables in many of the works cited above: Participation in collective political action is assumed to be a function of intensity of demand for the public good, the effect of which is additive with respect to other variables: the more dissatisfied an individual is, the more likely he or she is to join in collective political protest, no matter what the value of any other variable may be.

The difficulty with the simple grievance model is that it ignores the influence of the individual on provision of the public good and thereby fails to recognize the free-rider problem. According to conventional rational choice theory, individuals participate if their net expected benefit from participation as compared with abstention is greater than zero. If individuals abstain, their expected benefit is

$$E(I) = p_n * V, \qquad (1)$$

where $E(I)$ is the expected benefit of inactivity or abstention, and p_n is the probability of success of collective political action without an individual's contribution. If individuals participate, their expected benefit is

$$E(A) = (p_n + p_i) * V \qquad (2)$$

where p_i is the probability that the individual's own actions will make a difference in the likelihood of obtaining the public good. If individuals are members of large groups, p_i should be approximately zero, because the participation of a single member is assumed to make no objective difference in the outcome. Subtracting (1) from (2) leads to a net expected benefit of participation of zero; consequently, if there are any costs of participation, the rational choice is to abstain, regardless of the level of V, intensity of demand for the public good.[3] By neglecting to weight demand for the public good by the individual's perceived personal influence on provision of the good, the simple grievance model entails the hidden assumption that p_i is equal to unity (1.0), which is obviously false. Given the more reasonable assumption of conventional rational choice theory (that p_i is approximately equal to zero), as p_n approaches unity, the rational individual with high V will ride free on the efforts of others to provide the public good.

While conventional rational choice theory focuses on the influence that each individual has in bringing about a collective outcome (p_i), many scholars, instead, advance the notion that perceptions of *group* influence are decisive. For example, Pinard and Hamilton (1986, 231) argue that "the lack of any expectancy of [group] success, more than any other mobilization problem, is . . . immediately responsible for the failure of innumerable groups . . . to act, groups which otherwise had the proper configurations of internal motives, particularly grievances, and external incentives." An emphasis on the likelihood of group success as a determinant of an individual's decision to participate in collective action is also found in many other recent conceptual and empirical papers on the collective action problem (e.g., Fireman and Gamson 1979; Klandermans 1984; Muller and Opp 1986; Oberschall 1980).

The free-rider dilemma presents itself most clearly, however, precisely when group success is most probable. If the group as a whole can actually succeed in providing a given public good, there is all the more reason for the given individual to stay at home and enjoy the benefits of the good, which will be provided in any case with virtually equal likelihood. If the group cannot succeed, there is no question that ordinary members will have no motivation to participate; but when the group can succeed, each individual is faced with the free-rider dilemma: Why should I absorb the costs of action to provide a collective good when the group will provide it for me? This is because the likelihood of group success, denoted p_g, will necessarily be closely related to p_n, the likelihood of the success of collective political action given that the individual abstains. Therefore, regardless of the level of both p_g and V, the rational choice is still to abstain, given that p_i is approximately equal to zero.

The problem is how to link demand for the public good and expectancy of group success to individual participation in collective political action by a theoretically convincing rationale that overcomes the logic of free-riding; that is, why should the individual participate if the likelihood of group success is high? We propose two solutions that can be tested. The first, and most obvious, is to relax the assumption that p_i is approximately equal to zero. The results of much empirical research show that many individuals believe that their own contributions do make a difference to the likelihood of success of collective political action (Moe 1980; Muller and Opp 1986; Opp 1988). In a general sense, the p_i term is similar to an individual's political efficacy, which is a function of resources such as education, income, occupational status, and prior political involvement (Verba and Nie 1972). Thus, those with a strong sense of general political efficacy may believe that they make a difference because of their resources and political expertise. Allowing for the presence of subjectively "resourceful" individuals, then, makes it tenable to assume that p_i can vary between zero and one.

However, individuals should not perceive expected benefits from participation in collective action unless feelings of personal efficacy are coupled with a perception that the group as a whole is likely to succeed. By contrast, if personal influence is low or if the group is expected not to succeed, there is no rational incentive for the individual to participate: in the former instance, free-riding (and hence abstention) is the rational choice; in the latter instance, abstention again is the rational choice, since the cause is hopeless. Thus, the first solution to the collective action problem hinges on a multiplicative interaction between personal influence and likelihood of group success. This personal influence model may be expressed as

$$E(A) = (p_i * p_g) * V,$$

where p_g is the extent to which the actions of the group as a whole are likely to be successful in providing the public good. The logic of the personal influence model is similar to what Klandermans (1984) calls the "collective motive" for willingness to participate in collective action.

The second solution to the collective action problem is provided when individuals act on the basis of general strategic or ethical beliefs that promote what can be termed "collective rationality" (Rapoport 1974). The source of these beliefs is the ubiquitous question in collective action problems, What if everyone did that? (Hardin 1982). If "everyone" acted in accordance with the assumptions of conventional rational choice theory, all would abstain and no public good would be provided. If "everyone" participated, on the other hand, the public good would have a chance to be provided. The individual, faced with this dilemma, may first reason strategically that the participation of everyone is *necessary* to have a chance of obtaining the public good, that is, that the group can succeed only through the contribution of *all* members. If individuals believe in this "unity principle," their actions' utility and that of the group as a whole become indistinguishable, and each individual will participate if the overall potential for group success appears high.

In rational choice terminology, to the extent that the individual believes in the unity principle, the perceived "minimum contribution set" for provision of the public good approaches the set of all group members (Rapoport 1985; Van de Kragt et al. 1983). If *all* group members are necessary for group success, then without the individual's contribution, the minimum contribution set is less than needed, and the public good will not be provided. In other words, the expected benefit from abstention *diminishes* to the extent that the individual believes in the necessity of group unity. Consequently, the net expected benefit from participation is

$$E(A) = (u * p_g) * V, \tag{3}$$

where u is the individual's belief in the unity principle, namely, that collective action can succeed only if all members contribute. Thus, the likelihood of group success is relevant to the individual's utility calculus only to the extent that he or she believes in the necessity of group unity.

A second source of "collective rationality" hinges on a moral response to the question, What if everyone did that? Individuals may believe not that they need to participate for the group to succeed but that they *should* participate because free-riding is collectively irrational. The moral duty to participate in providing public goods is often understood in terms of Kantian ethics, where we must "do what we can rationally will that everyone should do" (Elster 1985, 142). But unconditional Kantianism is unlikely in collective action situations; unless the action potentially can succeed, moral edicts to participate will have little practical force (Elster 1985; Hardin 1982, 118–22). The individual is more likely to be a "calculating Kantian," who "will do as he would have others do so long as there are enough others who do likewise to produce net benefits for him, but he will not adamantly contribute to a lost cause" (Hardin 1982, 118; see also Godwin and Mitchell 1982). In other words, the calculating Kantian is one who will "play fair" and cooperate if enough others are willing to do the same.[4] Individuals will then "do their duty" so long as the probability of group success appears likely, and the net benefits to the group from all acting morally are greater than the costs. The calculating Kantian then participates according to the following expression:

$$E(A) = (d * p_g) * V, \tag{4}$$

where d represents the sense of duty to contribute to providing a public good. Combining the models in (3) and (4) above leads to a full collective rationality model of

$$E(A) = [(u + d) * p_g] * V, \tag{5}$$

with all terms as defined above.

While the beliefs that promote collective rationality are extrarational from the view of conventional rational choice theory (Hardin 1982, chap. 7), they are nevertheless beliefs that have been long regarded as important for mobilizing groups to action. Schwartz (1976, 167), for example, has pointed out that protest organizations "almost invariably adopt slogans conveying the notion that 'unity is strength' "—like "United we stand, divided we fall"—which serve to instill the principles promoting collective rationality into group members. Roemer (1978, 154) argues that "the success of unions does not depend on coercion or side payments but on workers learning to discard the individualist model and adopting a collective rationality." And Gamson (1975, 60) notes that individuals may recognize that the sum of individually "irrational" contributions makes for collective rationality and thus they may participate "not in spite of, but *because of* the full force of Olson's argument."

In sum, we postulate that there are two ways in which the utility-maximizing individual with a high level of demand for a public good may reject the option of free-riding and participate in collective political action. One way is through the interaction of perceptions of personal influence on provision of the public good, p_i, and the perceived likelihood of overall group success, p_g. If individuals believe that the group can succeed but that they are not personally efficacious, free-riding is clearly the rational choice; but as perceptions of group success and personal influence increase, individuals believe that the public good can be provided and that their participation contributes significantly to its provision as well. The second way is through two beliefs that promote "collective rationality": the unity principle (u), which leads to the perceived strategic importance of each contribution; and the duty to participate (d), which leads to the perceived ethical importance of each contribution as well. Without these beliefs, grievances and perceptions of the likelihood of group success have little relevance for each individual's decision to participate in collective action. But given a strong belief in the necessity of group unity or a strong sense of duty to participate, the perception that the group can succeed leads to the strategic and moral urgency of each individual contribution.

We test these hypotheses with survey data collected from a national sample and two local samples in West Germany. Previous empirical tests of participation based on the logic of collective action have utilized only specialized samples of protest groups or community samples (Klandermans 1984; Muller and Opp 1986; Opp 1986); the research design here affords the first test of such models in a representative national survey. Since earlier research has shown that political protest encompasses two dimensions, one consisting of legal, and one consisting of illegal, behavior (Muller 1979; Opp 1988), we will conduct separate tests of the hypotheses in regard to participation in legal, as well as illegal, protest.

Research Design

Three surveys were conducted in the Federal Republic of Germany during November 1987 to January 1988. A total of 1,709 persons of age eighteen and older were interviewed. One survey is a representative sample of the national population ($N = 714$). In order to overcome the problem that political protest in general and illegal forms of protest in particular are rare phenomena, representative samples were drawn from two communities where many protesters were expected to live. One community survey includes 501 persons from the county (*Landkreis*) Schwandorf in Bavaria, a rural area where extensive legal and illegal protest has occurred in regard to the issue of nuclear power, focused on the highly controversial nuclear recycling plant under construction in Wackersdorf, a small town in this county. Populations with a high incidence of protest are also likely to be found in so-called counterculture areas, where many young people, particularly students, and adherents of the Green party live. For a second protest-prone area, we selected the district Bockenheim of Frankfurt, where a third sample of 494 persons was drawn. This three-sample design thus affords an opportunity for rigorous testing of the generalizability of the models we have outlined.

The data were collected by the GFM-GETAS survey research institute in Hamburg, a firm with expertise in designing and implementing surveys on protest and political participation. Each survey was a probability sample drawn according to the design of the Working Group of German Market Research Institutes (ADM master sample). In this procedure the first step is to select sample points (voting districts)—for example, there were 210 in the representative national sample. Then the interviewer looks for households according to a random route procedure. Finally, a member of the household is randomly selected to be interviewed.

Measurement

In this section, we describe the construction of the scales to be used in the analyses. The means, standard devia-

Table 1. Means, Standard Deviations and Value Ranges of the Variables

		National Sample		Schwandorf		Frankfurt	
Variable	Possible Range	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Protest							
Legal	1–15	3.13	2.61	2.58	2.11	3.75	2.92
Illegal	1–15	1.26	.74	1.13	.42	1.52	1.18
Policy dissatisfaction	0–20	10.15	4.49	6.39	4.85	12.24	4.59
Past group success							
Legal	0–1	.64	.21	.58	.17	.65	.21
Illegal	0–1	.24	.22	.22	.21	.30	.24
Willingness of others							
Legal	1–5	2.56	.82	2.45	.74	2.71	.78
Illegal	1–5	1.33	.59	1.17	.36	1.62	.75
Likelihood of group success							
Legal	0–1	.35	.16	.30	.14	.37	.16
Illegal	0–1	.07	.08	.05	.05	.12	.12
Perceived personal influence							
Legal	0–1	.46	.26	.24	.27	.48	.25
Illegal	0–1	.15	.21	.04	.11	.22	.24
Belief in the unity principle	0–1	.67	.24	.56	.27	.71	.22
Duty to participate	0–1	.59	.29	.45	.29	.62	.32
Collective rationality scale	0–1	.63	.21	.50	.24	.66	.20
Number of cases		714		501		494	

tions, and value ranges for all variables for all three samples are shown in Table 1.

Legal and illegal protest. The dependent variables were measured following a procedure developed by Muller (1979). Respondents were questioned about their past performance of a series of legal and illegal behaviors (*never, once, several times*) and about their future intention to perform these behaviors (five categories, from *not at all* to *very likely*). The set of legal items includes signing a petition, taking part in a permitted demonstration, wearing a button or a sticker for a political cause, working with a citizen's action group, and collecting signatures for a petition. The illegal items include taking part in a demonstration that breaks the law; seizing buildings, such as factories or government or university offices; participating in confrontations with police or other governmental authorities; participating in political activities that may result in property damage (e.g., breaking windows or damaging construction sites or vehicles); participating in illegal protest activities at the work place (e.g., wildcat strike, sabotage, slowdown, etc.); participating in confrontations with other political groups or individuals; seizing building sites; and taking part in public disorders (e.g., blocking streets, sit-ins, etc.). To ensure anonymity and therefore enhance the validity of the answers, these questions were asked in the form of a self-administered questionnaire. The respondent placed it in a separate envelope that was then sealed and given to the interviewer.

Each behavior response was multiplied by the respective intention response. The resulting product terms were subjected to a factor analysis (unweighted least squares [ULS], varimax rotation). Two factors were extracted, one exhibiting high loadings only of the legal protest product terms, the other showing high loadings only of the illegal protest product terms. A legal protest scale and an illegal protest scale were constructed by adding the product terms of the legal and illegal items respectively and dividing each scale by the number of items. Because of the skewed distribution for both the legal and illegal protest, we use the natural logarithm of the scales in the correlational and regression analyses that follow (see Muller 1979, 54). In Table 1, however, we report the raw scores for ease of interpretation.

As the table indicates, legal protest is highest in the Frankfurt sample, the counterculture area; whereas Schwandorf has the lowest level. Protest behavior in that rural and mostly conservative region is generally low, despite the activities in recent years concerning the controversial recycling plant in Wackersdorf. The standard deviation of legal protest is also highest for Frankfurt, suggesting that there are many people there with a relatively high level of protest behavior. Illegal protest shows a similar pattern: Frankfurt is highest (again with the highest standard deviation), and Schwandorf is lowest. In general, the mean of legal protest and its standard deviation are much higher than the mean and standard deviation of illegal protest in every sample.

Policy dissatisfaction. Respondents were asked to what extent they were concerned about (1) extent of crime, (2) extent of unemployment, (3) the differences between rich and poor, (4) the cost of living, (5) problems in the community of the respondent, (6) environmental pollution, (7) nuclear power stations, (8) deployment of missiles, (9) number of foreigners. There were five response categories, from *not at all concerned,* coded zero, to *extremely concerned,* coded four. For each issue the respondent was asked to rate the government's performance, again with five categories, from *excellent,* coded one, to *very poor,* coded five. If the respondent thought that dealing with the issue was not a task of the government, the value zero was assigned.

For each issue, measures of concern and dissatisfaction with government performance were multiplied. A high value of a product term means that a respondent is both concerned about an issue and dissatisfied with the policies of the government for dealing with it. This method of measuring policy dissatisfaction is similar to the procedure used by Barnes and Kaase (1979) and Muller (1979).

The nine product terms were subjected to a factor analysis (ULS with a varimax rotation). Two factors were extracted. On the first factor items 2, 3, 6, 7, and 8 exhibited high loadings; on the second factor items 1 and 9 loaded highly. Items loading highly on the first factor were added to form a policy dissatisfaction scale, and this scale was then divided by the number of items. As expected, Table 1 shows that policy dissatisfaction was highest in the Frankfurt sample.

Perceived likelihood of group success (p_g). The scales for perceptions of the likelihood of group success through legal and illegal protest were constructed by multiplying measures referring to past group success (see Muller 1972, 1979) and present willingness of others to participate in these kinds of activities. To measure *past group success,* respondents were asked to indicate to what extent the following legal and illegal actions of political groups in West Germany had "helped their cause": collecting signatures (legal), taking part in legal demonstrations (legal), seizing buildings (illegal), blocking streets or participating in sit-ins (illegal), and participating in confrontations with police or other government authorities (illegal). There were five response categories, ranging from *hurt a lot* to *helped a lot.* Two scales, past group success by legal protest and past group success by illegal protest, were constructed by averaging responses to each set of items and were transformed to range from zero to one.

We improved previous measures of group success by including questions designed to measure the *willingness of others to protest.* Respondents were asked to estimate how many people living in their area with political views similar to theirs would be willing to get involved in collecting signatures for a petition (legal), working with a citizen's action committee (legal), seizing buildings (illegal), blocking streets or participating in sit-ins (illegal). The response categories were *none, some, a few, many,* and *almost all.*

We assume that the effect of past group success on protest behavior depends on the number of others presently perceived to be willing to protest. We therefore constructed a general likelihood of group success measure for legal and illegal protest by multiplying the measures of past group success and willingness of others to protest and transforming this index to a scale of 0 to 1. High values refer to higher perceptions of the likelihood of group success.

Personal influence (p_i). The respondents were asked to indicate the extent to which they personally could influence politics if they were to perform the following legal and illegal actions: collecting signatures for a petition (legal), working with a citizen's action committee (legal), seizing buildings (illegal), blocking streets or participating in sit-ins (illegal). Five response categories, ranging from *would have no influence* to *would have great influence* were presented. Two additive scales, influence by legal protest and influence by illegal protest, were constructed from the two items corresponding to legal and illegal behaviors respectively; and each scale index was transformed to a scale of zero to one, so that each can be interpreted as a probability estimate.

Belief in the unity principle (u). Respondents were asked to agree more or agree less (on five-point scales) with the following items: (1) for groups to have a reasonable chance of success by means of political actions everyone must contribute a small part; and (2) every individual member is necessary for the success of a political group, no matter how large it is. The items were added and divided by the number of items, and the index was then transformed to the 0 to 1 scale used for the previous items.

Duty to participate (d). Respondents were asked to agree more or agree less (on a five-point scale) with the following item: if a citizen is dissatisfied with the policy of the government, he or she has a duty to do something about it. The item was also transformed to a zero-to-one scale.

Collective rationality scale. We combined belief in the unity principle and belief in the duty to participate into a single scale for use in testing the full collective rationality model in equation 5. The correlation between belief in group unity and the duty to participate items in the full sample is .33, indicating a moderate relationship between the two components of the scale. The scale was also transformed to range from zero to one.

The distributions of the influence terms and the beliefs promoting collective rationality in Table 1 show a pattern similar to the other variables': Frankfurt has the highest values and Schwandorf the lowest. It is noteworthy in general that the influence terms, the belief-in-the-unity principle, and the duty-to-participate item have means clearly exceeding zero and in particular that the means of the influence terms referring to legal protest are higher than the means of the influence terms referring to illegal protest.[5]

Missing values were substituted by the arithmetic means of the variables. In general there were very few missing values. If

Table 2. Correlations between Protest Participation and Policy Dissatisfaction, Group Success, Personal Influence, and Beliefs in Collective Rationality

	Legal Protest			Illegal Protest		
Variable	National	Schwandorf	Frankfurt	National	Schwandorf	Frankfurt
Policy dissatisfaction	.35	.50	.51	.27	.28	.41
Group success						
Legal	.36	.54	.48	.13	.21	.35
Illegal	.23	.07*	.48	.41	.27	.56
Personal influence						
Legal	.33	.58	.28	.17	.29	.18
Illegal	.29	.26	.39	.44	.38	.45
Belief in the unity principle	.23	.52	.20	.08	.29	.10
Duty to participate	.28	.49	.31	.17	.25	.21
Collective rationality scale	.34	.58	.36	.16	.31	.22
Number of cases	714	501	494	714	501	494

*Not significant at the .05 level.

more than 5% of the cases were missing (which occurred with only one item), correlations with the dependent variables were computed with and without replacements of missing values. Since the results were virtually identical, we decided in favor of replacement.

Results

We present first the bivariate correlations between the logs of legal and illegal protest and all independent variables for the three samples in Table 2. All of them provide evidence that policy dissatisfaction, perceptions of group success, personal influence, and the beliefs promoting collective rationality are significantly related to both forms of protest. Policy dissatisfaction shows moderate bivariate relationships with legal and with illegal protest, although in all samples the correlation with legal protest is larger. These correlations are very similar to those reported in previous surveys of West German and West European publics regarding the relationship between discontent with government policies and political protest (Barnes and Kaase 1979; Muller 1979; Opp 1988), and their size and consistency help explain the popularity of the grievance model in explaining individual protest participation.

The relationships between the natural logs of legal and illegal protest and the personal influence terms are also of moderate magnitude, and, more important, support the assumption of rational choice theory that individuals will choose the kind of action they regard as most effective in achieving their goals. This can be seen most clearly by comparing the magnitude of the correlation of each of the influence terms with legal and illegal protest. For every correlation in Table 2, the personal influence and group success terms for legal protest correlate more strongly with legal, than with illegal, protest; while the opposite is true for every illegal influence term. For example, in the national sample, the individual's perceived influence for legal protest correlates more strongly with legal (.33), than with illegal, protest (.17); and the perceived personal influence term for illegal protest is stronger for illegal (.44) than for legal (.29). Similarly, looking down the columns in Table 2 shows that legal protest is more strongly related to the personal and group influence terms for legal, than for illegal, protest in all samples but Frankfurt; and illegal protest is more strongly related to the influence terms for illegal, than for legal, protest in all three samples. While the magnitude of the coefficients varies across the three samples, the pattern of personal and group influence variables correlating more strongly with the appropriate kind of protest is consistent.

Finally, the extent of belief in the unity principle and the duty to participate, as well as the combined collective rationality scale, are moderately related to participation in collective political action. The strongest relationships in five of the six samples are seen for the collective rationality scale, which shows higher correlations than either of its two indicators taken separately. The collective rationality scale and the separate items also show higher correlations in all samples with legal, than with illegal, protest. Evidently, those who believe either in the unity principle or a sense of duty to participate are more likely to engage in legal than in illegal protest. This is most likely due to the fact that the measures of belief in the unity principle and the duty to participate did not stipulate a distinction between legal and illegal behavior, and individuals probably responded with legal protest in mind.

The relative merits of the alternative models described previously can be evaluated by a logarithmic procedure commonly used in econometrics to test models with multiplicative or interaction terms (see, e.g., Kelejian and Oates 1981). The procedure uses the logarithm of all variables as opposed to their raw values to obtain the coefficients of interest in a regression model. For example, the personal influence model, which contains an interaction between group influence (p_g), personal influence (p_i), and policy dissatisfaction (V) as the independent variables, can be written as

$$\text{Protest} = a * V^b * p_g^c * p_i^d * e, \qquad (6)$$

where a is a constant and b, c, and d are coefficients representing the effects of policy dissatisfaction, group influence, and personal influence, respectively, and e is the error term. Taking the logarithm of both sides of the equation yields

$$\log(\text{Protest}) = \log(a) + b * \log(V)$$
$$c * \log(p_g) + d * \log(p_i) + \log(e),$$

which can be estimated through the normal ordinary least squares (OLS) regression technique.

This procedure has several major advantages. First, the effect of each individual component of the interaction terms can be estimated, unlike the standard regression procedure when the entire product term is treated as a composite measure. Second, the standard regression procedure handles interaction by arbitrarily assuming that b, c, and d are one. The logarithmic procedure allows these coefficients to vary, however, and hence conveys information regarding different possible functional forms of the relationships (Berry and Feldman 1985, 60–63). Because the values b, c, and d are exponents in the multiplicative model in equation 6, they may be interpreted as follows: if the values are greater than one, this indicates a progressively increasing impact on protest as the values of the independent variable become larger, holding other variables constant; if the values are between zero and one, this indicates a decelerating functional form, where the impact increases at first and then levels off; if the value is one, this reduces to a constant marginal effect; and if the value is zero, the variable drops out of the equation completely (as $x^0 = 1$). A more straightforward interpretation of the unstandardized effects, however, is that each regression coefficient is a measure of elasticity, representing the *percentage* change in Y that is brought about by a *percentage* change in X (Hanushek and Jackson 1977, 98).

The various models of protest are tested by the regression equations shown in Table 3. In all equations for legal protest, we include the relevant perceptions of individual and group influence through *legal* protest; and in all illegal protest equations, we use the corresponding perceptions of influence through *illegal* means. The results for the grievance models show significant effects of policy dissatisfaction on legal and illegal protest in all samples. The effect is greater for legal than for illegal protest. In addition, the coefficients in the majority of the equations are substantially less than one, indicating a decelerating functional form of the relationship between dissatisfaction and protest; the impact of dissatisfaction levels off as its value becomes higher. Only in the equation for legal protest in Frankfurt does the effect of dissatisfaction on protest approach a linear function. In general, though, the accuracy of prediction for these equations (adjusted R-squared) is low in the national sample, is moderate for legal protest only in Schwandorf, and is moderate for both kinds of protest in Frankfurt.

The results for the personal influence model are also shown in Table 3. Here, the interaction of policy dissatisfaction (V), perceived likelihood of group success (p_g), and perceived individual influence (p_i) dramatically improves the fit of the model in every sample, for both forms of protest. The adjusted R-squared for the personal influence equations are all substantially larger than those for the grievance equations, reaching a high value of .49 for legal protest in Schwandorf. In addition, the effects of all variables are statistically significant and of relatively large substantive magnitude. Interestingly, for legal protest, the unstandardized coefficient for group success is always greater than one, indicating that a 1% change in this variable leads to a greater-than-1% change in protest, holding other variables constant. The marginal effect of personal influence on legal protest is closer to one, while the effect of dissatisfaction is again less than one. Group success also shows a stronger relationship than personal influence with illegal protest, although the large accelerating effect is seen only in the Frankfurt sample.

The results from the personal influence model support the hypothesis that demand for public goods motivates individuals to contribute to collective action when both perceptions of individual influence and the overall likelihood of group success are high. If the perceived probability of group success is high, the personally inefficacious individual will be more likely to abstain; while those with higher levels of personal influence will be more likely to participate. Similarly, if perceived personal influence is high, those who believe the group has little chance of success will be more likely to abstain; while those who believe that the group has a higher overall likelihood of success will be more likely to join the collective action. The interaction of these two influence terms is strong and significant for both forms of protest in all three samples tested.

The results for the collective rationality models are also shown in Table 3. For legal protest, these models receive strong support, although accuracy of prediction is not improved over that for the personal influence equations. The distinctive components in these models are u (belief in the unity principle) and d (duty to participate), both of which are significantly related to participation in all three samples. The magnitude of the effects of group unity and duty are slightly smaller than that of personal influence for the national sample and Schwandorf, while

Table 3. Tests of Grievance, Personal Influence, and Collective Rationality Models of Legal and Illegal Protest

	Legal Protest (Log)			Illegal Protest (Log)		
Model	National	Schwandorf	Frankfurt	National	Schwandorf	Frankfurt
Grievance						
Log (V)	.41[a] (8.52)[b] *.30*[c]	.33 (11.68) *.46*	.76 (11.40) *.46*	.13 (5.95) *.22*	.06 (5.62) *.24*	.37 (8.56) *.36*
Adjusted R^2	.09	.21	.21	.05	.06	.13
Personal influence						
Log (V)	.27 (5.95) *.21*	.19 (7.66) *.26*	.55 (8.53) *.33*	.05 (2.39) *.08*	.05 (4.41) *.18*	.18 (4.55) *.17*
Log (p_g)	1.36 (6.43) *.24*	1.69 (7.34) *.27*	2.05 (8.09) *.33*	1.13 (6.98) *.26*	.78 (3.84) *.17*	1.78 (9.49) *.40*
Log(p_i)	.75 (5.51) *.20*	1.26 (10.55) *.39*	.42 (2.51) *.10*	.58 (8.10) *.30*	.69 (6.24) *.28*	.45 (4.42) *.19*
Adjusted R^2	.21	.49	.33	.25	.19	.37
Collective rationality (unity principle)						
Log (V)	.26 (5.59) *.20*	.20 (8.09) *.29*	.56 (8.60) *.33*	.07 (3.38) *.12*	.05 (4.63) *.20*	.21 (5.26) *.20*
Log (p_g)	1.62 (7.99) *.28*	2.00 (8.52) *.32*	2.17 (8.96) *.35*	1.69 (11.06) *.39*	1.19 (6.21) *.25*	2.17 (12.70) *.49*
Log (u)	.68 (4.23) *.15*	1.13 (8.19) *.30*	.50 (2.54) *.09*	.02 (.24) *.01*	.27 (4.79) *.20*	-.002 (-.02) *-.001*
Adjusted R^2	.20	.45	.33	.18	.17	.34
Collective rationality (duty to participate)						
Log (V)	.26 (5.58) *.20*	.20 (7.99) *.28*	.52 (7.95) *.31*	.06 (2.85) *.10*	.05 (4.83) *.21*	.20 (4.88) *.19*
Log (p_g)	1.65 (8.20) *.29*	2.13 (9.18) *.34*	2.15 (9.06) *.35*	1.67 (11.00) *.38*	1.22 (6.33) *.26*	2.13 (12.58) *.49*
Log (d)	.56 (4.55) *.16*	.89 (7.54) *.27*	.55 (4.32) *.16*	.16 (2.92) *.10*	.19 (3.85) *.16*	.14 (1.83) *.07*
Adjusted R^2	.20	.44	.35	.19	.16	.35

TABLE 3 (*continued*)

	Legal Protest (Log)			Illegal Protest (Log)		
Model	National	Schwandorf	Frankfurt	National	Schwandorf	Frankfurt
Full collective rationality						
Log (V)	.24	.19	.51	.06	.05	.20
	(5.04)	(7.65)	(7.91)	(2.83)	(4.26)	(4.92)
	.18	*.27*	*.31*	*.10*	*.18*	*.19*
Log (p_g)	1.54	1.79	2.05	1.67	1.17	2.13
	(7.63)	(7.72)	(8.58)	(10.99)	(6.14)	(12.43)
	.27	*.29*	*.33*	*.38*	*.25*	*.48*
Log ($u + d$)	1.02	1.46	1.09	.17	.31	.21
	(5.72)	(9.68)	(5.01)	(2.09)	(5.10)	(1.58)
	.20	*.36*	*.19*	*.07*	*.22*	*.06*
Adjusted R^2	.22	.48	.36	.19	.17	.35

Note: V = value of public goods (policy dissatisfaction); p_i = index of personal influence through legal or illegal protest; p_g = perceptions of group success through legal or illegal protest; u = belief in the unity principle; d = duty to participate.

[a] Unstandardized coefficients.

[b] t-ratio in parentheses.

[c] Standardized coefficients in italics.

all three effects are of roughly equal magnitude in Frankfurt. The general pattern is clear: given moderate-to-high levels of policy dissatisfaction, the more perceptions of the likelihood of group success are coupled with either the belief in the unity principle or in a duty to participate, the greater the likelihood that the individual will join a collective legal protest.

The equation with the highest accuracy of prediction for legal protest is the full collective rationality model, where the unity principle and the duty to participate are combined into a single collective rationality scale. Here the explained variation is greater than that of models with either indicator taken separately in all samples, and superior to that of the personal influence model in the national sample and in Frankfurt. The effect of the collective rationality scale is also quite large in unstandardized terms, larger than that of personal influence in all three samples, and rivaling in magnitude that of the likelihood of group success in all samples but Frankfurt. For legal protest, then, the beliefs promoting collective rationality are consistently relevant in explaining individual participation.

For the illegal protest equations, however, the collective rationality models receive little support. In Schwandorf, both variables taken separately show significant impact on protest; in the national sample only the duty to participate is significant; and in Frankfurt neither effect is significant. In addition, the magnitude of the effects are all quite small compared to their corresponding effects on legal protest and to the effects of personal influence for illegal protest. Combining the two collective rationality measures into a single scale similarly results in a relatively weaker model than that of personal influence. In Frankfurt, the combined index is not statistically significant; and in the national sample and Schwandorf, the impact of the scale is far weaker than that of either personal influence or the perceptions of the likelihood of group success. We conclude that for illegal protest, support exists primarily for the personal

Table 4. Full Model of Personal Influence, Collective Rationality and Legal and Illegal Protest

	Legal Protest (Log)			Illegal Protest (Log)		
Variable	National	Schwandorf	Frankfurt	National	Schwandorf	Frankfurt
Log (V)	.23[a]	.16	.50	.04	.03	.17
	(4.84)[b]	(7.04)	(7.82)	(1.86)	(3.09)	(4.39)
	.17[c]	*.23*	*.30*	*.06*	*.13*	*.17*
Log (p_g)	1.23	1.24	1.89	1.12	.72	1.77
	(5.83)	(5.42)	(7.82)	(6.94)	(3.61)	(9.44)
	.21	*.20*	*.30*	*.26*	*.16*	*.40*
Log (p_i)	.62	.99	.33	.58	.64	.44
	(4.49)	(8.30)	(2.00)	(8.00)	(5.82)	(4.18)
	.16	*.30*	*.08*	*.30*	*.26*	*.18*
Log ($u + d$)	.85	1.08	1.04	.13	.27	.10
	(4.74)	(7.25)	(4.77)	(1.72)	(4.61)	(.75)
	.17	*.26*	*.18*	*.06*	*.19*	*.03*
Adjusted R^2	.24	.54	.36	.26	.22	.37

Note: V = value of public goods (policy dissatisfaction); p_i = index of personal influence through legal or illegal protest; p_g = perceptions of group success through legal or illegal protest; u = belief in the unity principle; d = duty to participate.

[a] Unstandardized coefficients.

[b] t-ratio in parentheses.

[c] Standardized coefficients in italics.

influence model, while both the personal influence and collective rationality models are supported for legal protest.

The results of a full model combining personal influence and collective rationality are shown in Table 4. These equations include both personal influence and the collective rationality scale in a multiplicative specification. While the effects of the two variables may be additive, it is also plausible to assume that the effects of each may be augmented by the others. We decided on the multiplicative specification so as to maintain consistency with the equations in Table 3 and because additive specifications yielded no better fit to the data and made it almost impossible to interpret the individual effects due to severe multicollinearity.[6]

For legal protest, the full model shows that in each sample all variables have a statistically significant impact on participation. The relative magnitude of the unstandardized and standardized coefficients suggests that the effect of collective rationality is approximately equal to that of personal influence in the national sample and Schwandorf, while in Frankfurt collective rationality has a much greater effect. For illegal protest, however, the results of the full model are almost identical to the personal influence equations in Table 3, as collective rationality again adds little explanatory power.[7] Only in Schwandorf is the collective rationality scale significant, but its unstandardized value is low, once perceptions of personal influence are taken into account. Belief in the unity principle and moral duty, then, appear relevant only for legal protest: for discontented individuals, participation in illegal protest results almost entirely from the interaction between perceived personal influence and perceptions of the likelihood of group success.

Conclusion

Previous research on determinants of individual participation in collective political action has not dealt adequately with the free-rider problem posed by conventional rational choice theory. Grievance explanations ignore utility calculations and the free-rider dilemma completely. Explanations based on the perceived likelihood of group success fail to provide a meaningful linkage between perceptions of group success and the participation of any given individual. In our alternative models, one such linkage is specified by an interaction between the perceived likelihood of group success and perceptions of individual influence on the provision of public goods. We found substantial variation among West German respondents in their perceived personal influence, and a moderate-to-strong impact of this variable in the multiplicative interaction model of legal and illegal protest behavior.

A second linkage between the group and the individual is specified by an interaction between the perceived likelihood of group success and belief in either of two principles that promote collectively rational outcomes: the unity principle, which stipulates that the participation of all members is *necessary* for group success; and the duty to participate, which mandates individual participation on *moral* grounds. Given acceptance of the unity principle, individuals who perceive that a protest group can potentially succeed will participate because group success depends on each individual's contribution. Given acceptance of the duty to participate, individuals who perceive that a protest group can potentially succeed will participate because of the moral obligation to contribute to the provision of public goods if others are doing the same. Empirically, we found substantial variation among respondents in these beliefs as well, and a significant multiplicative effect of them on legal, but not illegal, protest in all three West German samples.

The results reported here have important implications for both grievance and rational choice theories of political participation. Discontent in the form of policy dissatisfaction does matter for protest, but it does not provide strong motivation for participation when considered alone. Participation in collective action results when dissatisfied individuals regard the group as likely to succeed, and when either perceptions of personal influence or belief in collectively rational general principles are high as well.

The implications for rational choice theory are much more complex. In contrast to the assumptions of the conventional rational choice model, many individuals believe that they *are* personally influential in providing public goods. Many individuals, further, believe—and act on—the unity principle, which stipulates illogically that *all* group members are necessary for group success, that is, if the individual does not participate, the group will fail (Hardin 1982, 114). Finally, individuals appear to act on the basis of moral obligations and feelings of duty to contribute if enough others are doing the same, and this motivation typically is interpreted as evidence against rational choice (Hardin 1982; Klosko 1987b).

However, the irrationality of these beliefs, and of acting on the basis of these beliefs, is not so clear. Consider the assumption of rational choice theory that individuals estimate their influence on the provision of public goods as close to zero. On the one hand, individuals who participate in large groups should not believe that their participation is decisive. On the other hand, high levels of perceived influence are found typically among individuals who are highly educated, wealthy, and cognitively sophisticated; while low levels of influence are found among those with fewer objective personal resources. Consequently, "miscalculation of individual influence cannot be assumed to be just an aberration due to lack of knowledge" (Muller and Opp 1987, 562); rather, such estimations appear to reflect perceptions of political influence and resources that can be relevant in stimulating individual and collective political behavior.

More generally, the processes we have outlined above conform to what Simon (1985) calls "procedural rationality," where imperfect information leads individuals to choose alternatives that may be characterized as inefficient according to some objective criterion. In this view, the criterion for assessing rationality is not the content of individuals' beliefs but rather that individuals act upon their beliefs in an efficient manner. Given this criterion, the results reported here are consistent with rational choice. On a basic level, the bivariate correlations in Table 2 show that perceptions of individual and group influence through legal protest are more strongly related to legal, than to illegal, protest participation, and that perceptions of influence through illegal protest correlate more strongly with that form of behavior than with legal protest. Thus, individuals appear to participate in whatever form of behavior promises the greatest chance of success. Moreover, the interaction effects demonstrated in the multivariate models are clearly compatible with procedural rationality. In the personal influence model, individuals who feel personally efficacious are unlikely to participate unless the group as a whole can also succeed, and individuals who believe that the group can succeed but feel personally inefficacious are likely to stay at home and free-ride. Thus, expectations of individual benefits from participation depend, quite reasonably, on the combination of personal influence and the likelihood of group success.

In the collective rationality model, efficiency in individual behavior also is evident. The effects of belief in the unity principle and the duty to participate are conditional on the perception that the group as a whole has the potential to succeed. Individuals who believe in the unity principle will be motivated to participate on strategic grounds, while calculating Kantians will contribute to collective action because the likelihood of group success both activates and augments the moral duty to participate. In both cases, individuals are maximizing utility based on their preferences for public goods and the constraints on realizing their preferences represented by their own perceptions of personal influence, acceptance or rejection of the beliefs promoting collective rationality, and the opportunities for successful collective action afforded by the environment.

Rational choice explanations of collective political action have always fared better in explaining why individuals do not participate rather than in explaining why they do. The processes described in the personal influence and collective rationality models show how procedurally rational individuals may calculate a positive expected utility for participation that outweighs the incentive to free-ride. Given these findings, the approach outlined here provides a useful explanatory framework for future research on the psychological determinants of participation in, and not simply abstention from, collective political action.

Notes

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1. Technically, the outcomes are *nonexcludable,* i.e., they cannot feasibly be withheld from any individual, regardless of his or her contribution toward its provision; and they have *jointness of sup-*

ply in that the good's supply does not change, regardless of how many people consume or enjoy it (Olson 1965; Oliver and Marwell 1988). The criteria of nonexcludability is the more important of the two for the purposes here.

2. Other conceptual and empirical critiques of grievance theory can be found in Finkel and Rule (1986), McCarthy and Zald (1977), and Tilly (1978).

3. It should also be mentioned that another solution to the collective action problem frequently mentioned by rational choice theorists is the provision of "selective benefits" to individuals—such as money, status, or "entertainment"—that can be obtained only through participation. While such selective benefits may be important, we focus here on the expected benefits associated with the collective good. We do not deny the importance of selective incentives but restrict our attention to the prediction of conventional rational choice theory that benefits associated with public goods are irrelevant for the individual's decision calculus.

4. On the role of fairness and moral obligations in collective action problems, see also Klosko 1987a and Marwell and Ames 1979.

5. It is also important to note that the intercorrelations between perceptions of individual influence, perceptions of the likelihood of group success, and beliefs promoting collective rationality are only of moderate magnitude, indicating that these variables represent distinct perceptions and attitudes that may influence participation.

6. That is, the interaction term $(p_i * p_g * V)$ is quite strongly related to $[(u + d) * p_g * V]$. In addition, using OLS in an additive model makes the unreasonable assumption, given our prior results, that the exponents of all variables in the interaction term are one.

7. Taking either of the two collective rationality terms separately yields the same conclusion.

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THE COOPERATIVE RESOLUTION OF POLICY CONFLICT

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I develop an approach for analyzing the conditions for cooperative resolution of policy conflict. I analyze certain policy conflicts as bargaining situations, with opportunity for cooperation, among opposing issue factions. As a framework for analysis, I present an informal game-theoretic interpretation of nonzero-sum policy conflict. With that foundation, I derive implications about the conditions for cooperative outcomes with respect to several aspects of the policy process: issue content, the structure of conflict, leadership, party politics, and political institutions.

I explore here an aspect of public policy making that students of U.S. politics have neglected: the conditions for constructive conflict resolution—or cooperation—between the opposing forces in policy conflict. Policy outcomes do not depend merely on the *relative* success of opposing forces. They often turn on whether those forces can agree to support policies designed to produce *mutual* gains: for example, whether liberals and conservatives agree on means to reduce budget deficits, whether consumer and agricultural interests work out inexpensive ways to stabilize farm income, and whether proindustry and environmentalist factions combine to promote efficient regulation. In effect, many policy conflicts resemble contract disputes between labor and management or diplomatic negotiations between nations at war. A central task for research on such conflicts, therefore, is to understand the conditions for cooperative outcomes.

First, I define the problem of cooperation in the context of policy conflict. Next, I suggest an analytic framework for explaining the degree of cooperation in such conflict. Finally, I use the framework to develop specific notions about the determinants of cooperative outcomes. The inquiry casts new light on several features of the policy process: issue content, the structure of conflict, leadership, party politics, and political institutions. Although the analysis is mainly concerned with the U.S. national government, it should also apply to other political systems; I point out some implications for cross-national comparisons.

The Problem of Cooperation

In many policy conflicts, the opposing factions have significant opportunity for cooperation; and the degree to which they exploit that opportunity is a crucial dimension of the policy outcome. As in the literature on negotiation, *cooperation* refers to constructive or mutually beneficial resolution of conflict.[1] Adapting Schelling's (1980) definition of a bargaining situation, we can say that a policy conflict offers opportunity for cooperation when two conditions are met. First, the factions engaged in debate must be interdependent. This occurs when each faction, alone or as part of a coalition, can affect the prospects for policy change. Such interdependence is absent only when one faction can act unilaterally.

Second, the factions must be in mixed motive, or nonzero-sum, conflict; that is, they must have both complementary and conflicting interests. To meet this test, the factions in a policy dispute may have common as well as conflicting goals. In the debate on the federal budget deficit, for example, liberals and conservatives have conflicting goals regarding taxes and spending but also have a common goal of reducing the deficit. The factions may have complementary interests with respect to means; some policies may serve their divergent goals, simultaneously, better than others (Lindblom 1965)). Economists argue that industry and environmentalists could both benefit through the adoption of market-oriented control strategies. In fact, any set of affected groups always has a common interest in eliminating inefficiency. If an inefficient policy also transfers income to some group, the other groups, in principle, can buy off the potential opposition by offering compensation (Tullock 1978).

If nothing else, the factions may differ in the importance they attach to various issues in dispute; they then can achieve joint gains by exchanging concessions accordingly (Raiffa 1982). The Immigration Reform Act of 1986 was largely such an exchange between labor, which wanted to reduce the future flow of illegal aliens, and Hispanic groups, which wanted to protect aliens already in the country (Perotti 1989). The combination of employer sanctions and an amnesty program was able to serve both objectives.

It will be useful to distinguish two kinds of nonzero-sum policy conflicts, much as we distinguish two-person and *n*-person games. In some cases the main conflicts divide two relatively coherent factions (which also have complementary interests). The factions may represent industry and environmentalists, the Sunbelt and the Frostbelt, liberals and conservatives, or other opponents. Even though less important divisions may also exist, I refer to these disputes as two-faction issues. In other cases, there are numerous factions that have largely distinct objectives (while also having a collective interest). For example, separate factions may represent the numerous industries affected by trade policy, economic groups affected by different tax provisions, geographic areas affected by public works programs, or the like. I refer to such cases as multifaction issues. In either kind of conflict, the factions can achieve joint gains by overcoming their conflicting interests and reaching agreement.[2]

Two observations will highlight the issue in this research. First, the cooperation possible in these cases goes beyond that in which groups with relatively compatible interests form a coalition to defeat other groups—for example, when labor and farm groups combine to extract benefits from consumers. I will call this narrow form of cooperation *competitive coalition building*. In a nonzero-sum policy dispute, there is opportunity for joint gain among the groups whose conflicts are central to the dispute, or even all of the groups affected by it. This is not only cooperation among certain groups but a cooperative resolution of the dispute.

Second, cooperation in this sense does not occur in policy decisions that are substantially without conflict, as in certain decisions about "the rules of the game" (Truman 1951), or where conflicting interests are ignored, as in some disaggregated policy making (Lowi 1966). It requires that conflicting interests be recognized and constructively resolved. Following Keohane (1984), I will call a situation without conflict one of *harmony*.

As the selective list in Table 1 indicates, issues that are nonzero-sum and present opportunity for cooperative resolution range from welfare reform and clean air regulation to budget deficits, tax reform, and immigration policy. Indeed, only a few active issues—such as abortion, capital punishment, and the level of defense spending—are essentially zero-sum.

Nevertheless, as Table 1 also indicates, the opportunities are often missed: budget deficits remain out of control; a destructive welfare system remains in place; inefficient regulations are not reformed. The key task for analysis, therefore, is to understand the variation: Under what conditions do the factions in a nonzero-sum policy conflict achieve a cooperative resolution—reaching agreement and adopting policies designed to produce joint benefits?

Procedurally, opposing factions have several ways to come together. There may be centralized negotiations. In the development of the Social Security rescue package in 1983, for example, the negotiations primarily involved the House Democratic leadership and the Reagan White House. There may be decentralized negotiations, perhaps taking place in several forums. Presidents, congressional leaders, or other entrepreneurs may perform a mediating role by designing proposals to win support from opposing factions. In

Table 1. Factions, Interests, and Outcomes in Selected Nonzero-Sum Policy Conflicts, Two-Faction and Multifaction

Policy Issue	Principal Factions	Conflicting Interests	Complementary Interests	Degree of Cooperation
Two-Faction				
Budget deficit (1983–88)	liberals vs. conservatives	social spending vs. defense spending and tax control	reduction of deficit through compromise	modest reductions despite several years effort
Welfare reform (1970–72)	liberals vs. conservatives	benefit increases vs. cost control	improve equity and work incentives through negative income tax	failure of Nixon administration proposals
Immigration reform (1971–86)	labor vs. Latinos	controlling immigration vs. protecting immigrants	an exchange: stronger enforcement but amnesty for current illegals	adoption of 1986 reform bill after lengthy delay
Air pollution control (1970–present)	environmentalists vs. industry	strengthening pollution control vs. minimizing costs	improve efficiency of regulation, as with market incentives	minimal progress, mainly through administrative means
Multifaction				
Trade policy (1934–present)	multiple industries subject to trade competition, others dependent on exports	demands for various tariffs and quotas	general economic benefits of free trade	general policy of trade liberalism, though with significant deterioration since late 1970s
Tax reform (1969–86)	multiple, overlapping groups affected by special tax provisions	special benefits of different groups, preference of some groups for lower rates	administrative and economic efficiency of simpler tax code	nearly uniform failure of proposals before substantial reform of 1986

some cases opposing factions may simply converge on a common position.

To assess the degree to which cooperation occurs requires looking at both the process of decision and the expected consequences of the resulting policy. Regarding the process, we can consider an outcome cooperative if, in a two-faction issue, large majorities of both factions support a measure or if, in a multifaction issue, majorities of most factions do so. Regarding the result, we can consider an outcome cooperative if most of the potential joint gain is actually achieved.

An outcome is not cooperative if, for example, two factions agree to extract distributive benefits from another faction or if one faction uses special benefits to buy off part of another faction: either represents competitive coalition building. Nor is an outcome cooperative (except minimally) if the factions agree without making concessions needed to achieve joint gains—a lowest-common-denominator agreement. This occurs, for example, in pork barrel programs characterized by "universalism" (cf. Weingast 1979). In any case, policy outcomes also have a distributive dimension—the factions' relative success—but that dimension is not the focus of this study.

How readily such cooperation occurs determines, in large part, the performance of the policy process. To be sure, some important policy changes, such as the adoption of major civil rights laws, have been possible only through conflictual outcomes. Other things being equal, however, a greater capacity for cooperation should produce a higher rate of policy change. It should lead to more efficient policy outcomes. And it should facilitate serving broadly based interests instead of narrow ones. As a result, such a capacity should also increase generalized support for the political system.

Nevertheless, students of U.S. politics have made hardly any effort to identify the conditions for cooperative resolution of policy conflict. To a great extent, political scientists have conceived of politics as zero-sum conflict.[3] The concept of power is often designated the central concept of political analysis (Apter 1977; Frey 1985), and most research on policy making has aimed to illuminate the relative power of different interests. Even the literature on coalitions mainly addresses competitive coalition building.[4]

In recent years, political scientists have begun to explore problems of cooperation in several contexts, such as the formation of interest groups (Moe 1980; Olson 1965; Wilson 1973), the evolution of norms (Axelrod 1986), and economic and security relations among nations (Keohane 1984; Oye 1986b). In comparative politics, research on democratic corporatism has brought this concern to the study of policy making (Katzenstein 1985). But there has not been an effort to develop a general theory of cooperative policy making or one that applies in the United States.[5]

Theoretical Framework

In this section I sketch an informal game-theory interpretation of nonzero-sum policy conflict.[6] First, I identify the actors in the analysis and discuss their behavioral properties. Next, I describe the environment of their interaction. Finally, I identify the kinds of games they are engaged in and the general determinants of the outcomes. In succeeding sections I use those determinants to help develop specific hypotheses.

Issue Factions

The central actors in the analysis must be the political entities whose cooperative or conflictual interactions best account for policy outcomes. They cannot be the two political parties, rival institutions like the House and Senate or Congress and the president, or opposing interest groups.

The main policy conflicts in the United States often take place *within* each party or institution; and interest groups alone lack the power to determine outcomes.

The analysis will therefore focus on opposing *issue factions.* I define an issue faction as an aggregation of policy makers and constituencies with relatively coherent policy objectives in a given debate (cf. Nagel 1975). Such a faction may consist mainly of public officials, or it may include active interest groups and attentive members of the mass public. Political analysts employ roughly this concept when, for example, they describe a policy dispute as dividing the Frostbelt and the Sunbelt or industry and environmentalists. Though issue factions are not formal organizations, they often have considerable coordination. On partisan issues, the opposing factions are linked with the political parties. In general, policy conflict is resolved cooperatively if and only if the opposing factions reach agreement.

Besides the factions, however, there are usually other potential actors who at a given time are uncommitted in the debate. Such third parties may even include, for example, the president or an important committee chairman. Because such actors may later enter the dispute, a faction's potential third-party support is a major element of its strategic situation.

A faction's response reflects three categories of motivation. First, the members of a faction seek certain economic, moral, or other benefits from public policy. To that extent, they regard specific policies as instrumental to those benefits. Second, they may have emotional or symbolic motivations, such as hostility to an opposing group or rigid commitment to an ideological position. As a result, they may also assign intrinsic value to certain policies or negotiating positions. Finally, the public or interest group officials who lead an issue faction also have political maintenance objectives such as reelection, career advancement, or organizational growth.

Because of this motivational complexity, a faction often deviates from the strategy likely to maximize policy benefits. In fact, there is evidently a general bias against cooperative strategies. In varying degrees, a faction may overestimate its political power. It may distrust leaders who promote compromise. It may fail to consider alternative policies. Or it may take satisfaction merely in defeating an opposing faction. To a great extent, such bias results from constituency pressures on faction leaders. Mass constituencies, in particular, are less cooperative than leaders because they are less informed about policy alternatives and strategic constraints, and more subject to emotional and symbolic motivation.[7] Some of the specific factors in the analysis primarily affect the severity of these biases.

The Environment of Policy Making

Four general features of the policy process in the United States define the factions' strategic environment. First, policy change normally requires broad agreement. Indeed, in policy conflicts that involve only a few factions, it often requires substantial support from each faction.[8] Second, in contrast with many kinds of negotiations, such change does not require unanimity. Especially in conflicts with numerous factions, a measure that some factions oppose or only partially support may still be adopted. Those factions that withhold support do not escape the measure's costs, such as loss of subsidy or of protection from competition, and may even end up with a larger share.

Third, in attempting to achieve cooperative outcomes, faction leaders can use explicit bargaining; they do not have to rely on fragile strategies of tacit bargaining like tit for tat.[9] The agreements they reach, though not legally enforceable, are generally reliable because politicians and interest group officials need the reputation of keeping their word. Relatedly, the availability of public officials to conduct negotiations reduces the costs of transactions among private groups (Frohlich, Oppenheimer, and Young 1971).

Fourth, a strategy of negotiation often entails serious risk. Significantly, the initial steps in a cooperative strategy are quite safe, even if not reciprocated. Faction leaders can send subtle cues of their willingness to negotiate and proceed only gradually to make concessions (see Pruitt 1982). If the other faction refuses to participate, little is lost by the attempt.

As a negotiation becomes serious, however, the risk increases. There is no assurance that agreement will be reached and policy change achieved. It may turn out that no mutually acceptable policy change even exists. Moreover, if negotiations fail after significant investment of time and modification of initial positions, the attempt may have significant costs. By pursuing negotiations, a faction may undermine the credibility of its demands; it may waste a short-lived opportunity to pursue its goals by conflictual means; or it may lose ability to mobilize constituencies. In addition, the leaders of one or more factions may suffer a loss of credit.

None of these losses depend on any faction's dissembling about its intentions (which is difficult anyway for such a complex entity) or otherwise exploiting the attempt to cooperate: the same losses can occur even if all sides enter negotiations in good faith and abandon the effort simultaneously. The risks of negotiation will sometimes constitute a major deterrent to cooperative policy making. Some of the specific factors in the analysis primarily affect these risks.

A Game Theoretic Interpretation

These observations help define the games in which competing factions are engaged. We look at the case of two issue factions in a nonzero-sum policy dispute. Each chooses, continuously, between a conflictual and a cooperative strategy. In a *conflictual strategy,* a faction seeks distributive gain (or avoidance of distributive loss) through defeat of the opposing faction. It makes ambitious demands, refuses to consider significant concessions, and withholds information about its priorities. It uses confrontationist rhetoric to mobilize constituencies and tries to pressure uncommitted politicians and other third parties for their support.

In a *cooperative strategy,* a faction seeks joint gains (or avoidance of joint loss) through agreement with the opposing faction. It tries to establish negotiations with the opposing faction or supports mediation efforts by third parties. As long as the other faction reciprocates, it makes moderate demands, considers significant concessions, and provides information about its priorities. Such a faction avoids confrontationist rhetoric and offers proposals designed to produce joint benefits. If agreement is reached, it joins the other faction in seeking adoption of a mutually acceptable policy change.

By defining the strategies to include the decision whether to negotiate and not merely whether to accept a cooperative measure, I focus attention on the uncertainties of the negotiation itself. To identify the conditions for cooperative outcomes, I will discuss three characteristics of the resulting games: the structure of perceived payoffs, the number of players, and the degree of iteration. It turns out that the perceived payoffs are most important.

Payoff structure. There are four possible *proximate* outcomes, or combinations of faction strategies, each of which represents a type of policy process (see Table 2). The ultimate result—what policy change occurs, if any—is uncertain until that process unfolds.

Table 2. Outcomes of Cooperative and Conflictual Strategies

	Strategy	
Strategy	Cooperative	Conflictual
Cooperative	negotiation (CC)	refusal (CD)
Conflictual	refusal (DC)	conflict (DD)

If both factions adopt conflictual strategies, it leads to a straightforward political battle. I call this *conflict* (or DD—defect-defect). This process may lead to victory by one faction or else to stalemate. A winning faction obtains distributive gains. There probably will be no joint gains, and, especially with stalemate, there may be joint losses.

The expected payoffs for *conflict* depend largely on the competing factions' balance of political resources and institutional or strategic advantages. However, a faction may overestimate its power and expect victory despite unfavorable objective conditions. Indeed, both factions may expect it. More fundamentally, the payoffs also depend on how easily institutions allow clear-cut victories.

The asymmetric outcomes are relatively innocuous. If one faction *cooperates* but the other *defects*, the outcome is *refusal* (CD or DC). There is a short feeling-out process followed by the resumption of conflict. Importantly, the defector does not reap a large reward, nor the cooperator a penalty. A cooperating faction can approach negotiation cautiously and refrain from supporting a cooperative measure until the other faction also adopts it. Instead of conferring an exploiter's profit, therefore, *refusal* leads to essentially the same process and resulting payoffs as *conflict.* This means that a faction can initiate cooperation without much risk of being exploited, and that policy conflicts do not amount to prisoners' dilemmas.

Finally, if both factions attempt a *cooperative* approach, the outcome is *negotiation* (CC).[10] The payoffs of this process depend on whether negotiation succeeds. If agreement is reached and action achieved, it will produce a policy change intended to yield joint benefits. Though perhaps in unequal degrees, both factions may expect to gain. If the factions fail to agree or other obstacles prevent action, no policy change will result. Such a breakdown, following full-scale negotiations, not only squanders opportunity for joint gain. Potentially costly to one or both factions, it may alter the balance of power or diminish the standing of faction leaders.

The perceived payoffs for negotiation depend on the amount of opportunity for joint gain, the costs and uncertainty of the negotiation, and the consequences of breakdown. Here again, however, a faction may have biased perceptions. An ideologically rigid faction may fail to recognize opportunity for joint gain that would require abandoning some of its demands.

Thus it appears that the main variations in the payoff structure turn on the factions' preferences between conflict and negotiation. Where they prefer negotiation, the entire ordering will be

$$CC > DC > DD > CD.$$

This payoff structure, which is called "stag hunt," is cooperation-stable in that if both players cooperate, they both get optimal payoffs (Downs, Rocke, and Siverson 1986). Moreover, because refusal has essentially the same payoff as conflict, the possibility of that outcome does not deter cooperative strategies.

Where a faction prefers conflict, however, the ordering is likely to be

$$DC > DD > CC > CD.$$

This payoff structure, which is called "deadlock," makes defection a dominant strategy and leads to conflict as a stable outcome (Downs, Rocke, and Siverson 1986). Finally, if each faction has one of these payoff structures, the outcome is refusal.

These differences in payoff structure are sufficient to explain variation in the character of policy making and policy outcomes. In fact, they leave the other game theory dimensions with modest roles.

Number of players and iteration. In many contexts, the chances for cooperation depend on the number of players (Hardin 1982; Olson 1965) or the degree to which interaction is iterated (Axelrod 1984). Neither dimension is crucial, however, in the relations among opposing issue factions.

The number of distinct factions in a dispute has ambiguous significance for the outcome (compare with Olson 1982). In many contexts, a multiplicity of players implies high information and transaction costs and strong incentives for free-riding. But in policy disputes, as we have noted, the mediating role of public officials reduces information and transaction costs; and the enforcement of law constrains free-riding. As Olson (1982) argues, if numerous groups have a collective interest, they will devote less than optimal effort to lobbying on behalf of it. But the same fragmentation also gives political leaders seeking a cooperative outcome numerous ways to assemble a winning coalition.

It is generally misleading, therefore, to analyze public policy disputes as *n*-person prisoner's dilemmas (compare with Olson 1982). And in fact, government often adopts policies—such as antitrust measures and trade liberalization—that serve collective interests of numerous groups. When important collective interests are overlooked, it is often because the groups have particular difficulty accurately perceiving those interests.[11]

At the level of interaction among issue factions, the degree of iteration also has mixed effects. Iteration permits the use of tacit bargaining through a series of responses to induce an opponent to cooperate (Axelrod 1984). This could imply that the more often the issues that divide a set of factions arise, the more likely cooperative outcomes are.[12] But tacit bargaining is not the principal basis of cooperation in this context. Where explicit bargaining is available, it supercedes the less-effective tacit bargaining, and makes the conditions for successful tacit bargaining largely irrelevant to cooperation.[13] With explicit bargaining, opposing factions are free to cooperate if and only if they find it mutually beneficial in a given dispute; and they can do so regardless of whether the dispute will be revisited.

In fact, the repetition of a policy conflict both aids and obstructs cooperation. On the one hand, such repetition often produces a stable and predictable balance of power, which will encourage cooperative strategies. But it also may cause polarization and mutual hostility; and it gives each faction incentive to stand firm to keep opponents from expecting easy concessions in future disputes. Issues that arise regularly, like income taxation or Social Security, do not seem markedly subject to cooperation.

At another level, however, iteration is indeed vital to cooperation. The iterativeness of the relations among politicians and interest group officials, as individuals, helps enforce the norms of truthfulness and promise keeping that make bargaining possible between issue factions (Axelrod 1986).

General Determinants of Cooperation

The analysis points to a handful of general determinants of cooperation. Cooperative outcomes are more likely when the competing factions expect that, (1) the potential benefits of cooperative policy change are large, (2) the obstacles to reaching agreement are modest, (3) the potential losses from a failed attempt to reach agreement are also modest, and (4) the gains available through a conflictual process are small or negative. These expectations partly reflect objective conditions of politics and policy. But they also may represent biased perceptions. Such perceptions result mainly from symbolic motivation or lack of information, especially on the part of faction constituencies, and they generally inhibit cooperation. To develop specific generalizations about the conditions for cooperation, I next apply these conclusions to various attributes of policy disputes and the political environment.

Conditions for Cooperation

The conditions relevant to this inquiry will involve the same features of the policy process that are examined from a different perspective in the standard literature on policy making. I will suggest conditions for cooperation with respect to issue content, the structure of conflict, leadership, party politics, and political institutions.

Issue Content

Lowi (1966) pointed out that certain attributes of a policy issue affect the scope of participation and the distribution of power. Other attributes affect the prospects for cooperation. The most obvious is the magnitude and salience of the potential joint gain from cooperative policy change: the larger and more salient the potential gains, the more likely a cooperative outcome is.[14] The opportunity for such gains increases the expected payoff for reaching agreement. A cooperative outcome is thus more likely if the opposing factions have highly valued common goals or marked differences in priorities across issues or if existing policies are highly inefficient.

This effect helps explain why liberal and conservative factions were able to reach agreement on a 1983 bill to eliminate the Social Security deficit but have failed to agree on measures to substantially reduce the federal budget deficit. A default of the Social Security program would have delayed benefit payments, a fiasco that both sides wanted to avoid at any cost. The damage caused by budget deficits has been uncertain and remote, and both factions have been willing to live with the consequences of intransigence.

A second aspect of issue content is the depth of the exchange needed to achieve joint gains. The depth of an exchange is the degree to which it involves concessions that—taken individually—are large or unexpected in the context of the dispute. In order of increasing depth, the forms of agreement possible are (1) compromise, where the factions make comparable, moderate concessions on each issue in dispute; (2) trade-off, where they exchange large or even complete concessions on different issues; (3) agreement by compensation, where one faction concedes the original issues and the other makes up for it on unrelated matters; and (4) agreement by reorientation, where both factions abandon their initial positions to adopt a fundamentally new alternative (see Pruitt 1982).

The deeper the exchange needed to produce joint gains, the harder it is to achieve agreement. If a faction is at all rigid, it will discount gains that require deep concessions. In any case it will expect a deep exchange to be difficult to negotiate and the negotiations to be subject to breakdown. Such an exchange often encounters the difficulty of linking issues in different institutional jurisdictions (Nivola 1986). Finally, because the negotiations will be lengthy and involve discussion of major concessions, the consequences of breakdown will be severe. Thus a policy conflict that permits joint gain through compromise or trade-off is more subject to cooperation than one requiring compensation or reorientation.

The difficulties of a deep exchange were much of the reason for the failure of the Nixon administration's Family Assistance Plan. Both liberals and conservatives had much to gain in the plan (Moynihan 1973). But its adoption would have amounted to agreement by reorientation, with liberals giving up benefit programs to be cashed out and conservatives accepting a massive increase in welfare spending and endorsing the principle of a guaranteed income. Instead, both factions responded with ideological posturing and impossible demands, and no bill was enacted. Similar considerations explain why efforts to reform anticompetitive regulatory programs rarely have relied on compensating regulated industries for the loss of protection (Derthick and Quirk 1985; Robyn 1987; see Tullock 1978).

The Structure of Conflict

There are three relevant aspects of the structure of conflict in a dispute. One is the balance of power among the factions. In a two-faction conflict, the more equal that balance, the more likely a cooperative outcome is. With equal power, neither faction can win large gains in a political struggle—which most likely will end in stalemate. The expected payoffs for conflict will be low. But if one faction is considerably stronger, it may expect a high conflict payoff and is likely to adopt a conflictual strategy.

Since the early 1970s, the balance of power between the environmental and proindustry factions on air pollution has fluctuated (Rosenbaum 1985). When one side has been on top—environmentalists in the early 1970s and industry in the early 1980s—it has aggressively sought competitive gains. When the balance has been more equal—as in the mid-1970s and mid-1980s—the tendency has been to work out deals with some benefits for both sides. In relation to this, one reason a policy dispute eventually becomes peaceful is that the factions acquire accurate estimates of their relative power.

In a multifaction conflict, a variety of coalition structures are possible and there is no determinate overall balance of power. There is, however, an initial partial balance between presumptive opponents and supporters of measures to serve collective interests. The opposition factions represent groups likely to bear disproportionate costs of such a measure (for example, industries threatened with the loss of major tax breaks). The support factions represent either groups that would receive special benefits (like export industries in trade liberalization) or broad constituencies such as consumers or taxpayers.

To the extent that the support factions are stronger, a cooperative outcome is more likely. This is true even if factions likely to receive roughly proportionate shares of the costs and benefits (say, industries with both import competition and significant exports) are predominant. Such a mixed interest faction will expect rewards, in the specific provisions of a bill, for joining a successful collective interest coalition, and penalties for opposing it or remaining neutral. Such factions will perceive the initial balance of power and will prefer to join a coalition with the stronger side.

One implication is that the prospects for cooperation in multifaction disputes depend partly on the distributive consequences of collective interest policies. One obstacle to tax reform, for example, has been that groups benefiting from special tax provisions are more identifiable and organized than those benefiting from lower rates (Witte 1985). For that reason, among others, the initial balance has favored opponents of tax reform. Proposals for trade liberalization fare better partly because industries that depend on trade are as identifiable and organized as those that suffer from it, even if less active (Baldwin 1985).

Two further aspects of the structure of conflict are related: the breadth of participation and the dimensionality of conflict.[15] First, the more participatory the opposing factions, the less likely a cooperative outcome is. I define a participatory faction as one whose attentive constituency includes mass-based organizations (like labor unions or citizens' groups), a large number of interest groups, or a significant segment of the general public. A nonparticipatory faction represents a small number of narrowly based interest groups (such as trade associations) or is dominated by elites. A participatory faction's constituencies generally will have a strong symbolic orientation and very limited information. They may overestimate the faction's power, be rigid in their demands, or take satisfaction in conflict itself. In addition, such a faction will have difficulty sustaining mobilization. It will be vulnerable to loss of mobilization through a cooperative strategy, and will expect to lose power in the event of breakdown. It is primarily for these reasons (and not because of transaction costs or free-riding) that cooperation is more difficult on issues that involve numerous interest groups.

Second, if the main conflict in a policy dispute corresponds to the left-right ideological cleavage, cooperation is less likely. Such disputes elicit symbolic motivation that favors conflictual strategies. In addition, because the left-right cleavage is the organizing principle of the party system, they create few cross pressures for politicians, who thus have little incentive to adopt moderate positions or act as mediators. For both reasons, the factions in left-right disputes tend to prefer conflict—which they may find rewarding, psychically or politically, even if it fails to produce policy benefits.

Many policy disputes have both participatory factions and left-right conflict. They often exhibit conspicuous failures of cooperation. Regarding occupational health and safety, for example, labor and industry could both benefit from a relaxation of certain very severe regulations combined with a strengthening of controls dealing with many hazards that are now essentially unregulated (Bryner 1987). But the two factions—each participatory, and divided between left and right—have not sought such an arrangement. Policy in the area has been mired in stalemate.

Other disputes have nonparticipatory factions divided along sectoral, geographic, or other lines. In general, they are more susceptible to cooperative outcomes. In technology transfer policy, for example, one faction promotes technology exports and another tries to preserve U.S. technological superiority over the Soviet Union. Neither is participatory, and the dispute cuts across ideological divisions. Policy making has proceeded largely through negotiation and mutual accommodation. There are also some issues that have left-right conflict and nonparticipatory factions (such as many issues of foreign policy) or participatory factions and conflict along another dimension (like the debate between labor and Latinos over immigration reform). Other things being equal, such issues are likely to be moderately difficult.

Leadership

Discussion of political leadership has stressed its significance for the relative power of competing interests (see, e.g., Burns 1978; McFarland 1969; Rockman 1984). It is also important for cooperation. Leaders negotiate for competing factions or mediate between them.[16] In doing so, they may reduce transaction costs by facilitating communication, increase agreement payoffs by helping to improve proposals, deter conflictual strategies by withholding support, or reduce the threat of breakdown by discouraging premature public commitments. The literature on negotiation and mediation suggests some skills and strategies that may enhance these effects (Perotti 1989).

One is that negotiators should adopt a posture of *flexible rigidity* (Pruitt 1982). They should be flexible regarding the means to achieve their goals. Such flexibility permits wide-ranging exploration of potential solutions. But they should be rather rigid about their level of aspiration. Such rigidity intensifies the joint effort to improve proposals. The more common failure in policy negotiations is undoubtedly lack of flexibility about means. But lack of appropriate rigidity also occurs—for example, when policy makers paper over serious differences or compromise issues indiscriminately. Excessive flexibility may bring peace without exploiting opportunities for joint gain.

Another suggestion is that leaders should direct negotiations through an ordered sequence of phases: (1) diagnosing the conflict, (2) devising a general formula for agreement, and (3) working out the details (Zartman and Berman 1982). The rationale for following the sequence is to simplify the task of designing an agreement and provide a framework to justify specific concessions. Interestingly, this approach argues against the common practice of initiating action by introducing a full-blown policy proposal.

Commentators also have advocated a procedure for mediation called negotiation with a single text (Zartman and Berman 1982). It requires the mediator to take responsibility for offering proposals. The parties criticize the proposals. And the mediator revises them until an acceptable solution is identified. The procedure manages to elicit detailed information about the parties' preferences without requiring them to make proposals or acknowledge concessions until agreement is at hand. It is used in policy making when a political leader assumes a neutral stance and introduces a series of proposals designed to be broadly acceptable. Committee chairmen in both houses used this approach in the development of the 1986 immigration reform bill.

Party Politics

Two aspects of party politics are central features of the policy process: the degree of party discipline, primarily in Congress, and the balance of party control in Congress and the White House. It is often supposed that a combination of disciplined parties and single-party control—party government—is most conducive to effective policy making (Fiorina 1988; Sundquist 1981). From the standpoint of cooperation among issue factions, however, the relationships are complex and vary with the issue at stake.

The parties seek to promote several dispositions among their members. These dispositions modify the payoffs that members perceive and the strategies they choose on behalf of issue factions. Significantly, the several dispositions have different effects for cooperation. First, because the parties are in some degree held collectively accountable for national conditions, they induce their members to serve broad national interests as opposed to narrow groups or geographic areas (Fiorina 1981, 1988). This orientation promotes cooperative outcomes, especially on multifaction issues. Second, however, each party reinforces allegiance to constituency groups, such as labor for the Democrats and business for the Republicans; in particular, they pressure moderate members to support constituency-oriented decisions of the party majority.

This tendency discourages cooperation on issues that divide Democratic and Republican groups. Finally, the parties conduct a collective rivalry for political credit.[17] The rivalry facilitates collaboration within the parties but inhibits it between them. The greater the party discipline, then, the more pronounced these effects are.

The balance of party control partly determines the strategic opportunities for opposing factions. In policy disputes that cut across party cleavages, this balance has only ambiguous effects: single-party control simplifies negotiation but also lowers the barriers to a conflictual strategy. In disputes that follow party lines, however, the effect is clear: single-party control implies a major power advantage for the faction aligned with the dominant party. It increases that faction's expected payoff for a conflictual strategy, and tends to preclude cooperation. When the Democrats control both Congress and the executive branch, for example, a prolabor faction is less likely to cooperate.

The conditions of party politics that promote cooperation, therefore, depend on the issue. Party government promotes cooperation in conflicts that involve multiple factions or that cut across party cleavages. In such cases, party government strengthens concern for national interests and facilitaties collaboration between the branches without enhancing conflictual strategies. In fact, a lack of effective party government undoubtedly has exacerbated the recent trend toward protectionist trade policy: With divided party control, congressional Democrats have exploited the issue to embarrass a Republican president. With undisciplined parties, individual entrepreneurs like Representative Gephardt have played a major role in putting protectionism on the agenda.

But party government militates against cooperation in conflicts that correspond to partisan cleavages. In such cases, it gives one faction the prospect of decisive victory. This effect was evident in the Johnson administration after the 1964 election, when liberal factions imposed their will to create programs; and in the early Reagan administration (despite nominal Democratic control in the House), when conservative factions did the same to cut programs. On partisan issues, cooperation is more likely under divided control, which implies a relatively equal balance of power, and weak parties, which give more leverage to moderate members.

Political Institutions: A Comparative Perspective

A nation's capacity for cooperative resolution of policy conflict depends in part on the structure of its political institutions. To make relevant comparisons, however, one must recognize that different institutions imply different processes for cooperation. Corporatist democracies rely on centralized bargaining among representatives of labor, industry, and government (Goldthorpe 1984; Katzenstein 1985; Schmitter 1974). The United States, in contrast, has ad hoc, decentralized bargaining and an environment of open debate. But either arrangement can lead to agreement and joint gains for competing factions.

Two dimensions of institutional structure affect cooperation among issue factions. One is the breadth of support required for policy change. Political institutions may respond to a narrow majority or require a broader consensus (Lijphart 1984). Consensual institutions make significant gains hard to achieve through conflictual strategies and therefore encourage cooperative outcomes. To that extent, any of several features that produce consensualism should also promote cooperation—in particular, structural divisions, like bicameralism, separation of powers, and federalism; electoral systems that give pivotal roles to centrist parties, like proportional representation; and formal requirements for extraordinary majorities (Lijphart 1984).

The other dimension is the complexity and exposure of decision making. Cooperation is easier with a simple and closed decision-making process—one that has few independent decision-making units, a small number of individuals directly involved in decision making, and limited publicity. (Needless to say, such processes encounter objections on other grounds). A simple, closed process lowers the costs of negotiation and improves the chances of implementing agreements; it insulates leaders from constituency pressures, giving them greater leeway to discuss concessions; and it reduces the potential costs of breakdown. Participants in such a process often develop mutual trust, which also facilitates reaching agreement. In short, consensualism favors cooperation by undermining conflictual strategies; simplicity and closedness do so by facilitating negotiation.

To a great extent, consensualism and simplicity are at odds. The institutions most conducive to cooperation, therefore, are those that require consensus and yet have simple and closed decision processes. This combination is often found in multiparty parliamentary systems. Such systems usually are led by centrist parties and have simple processes for making decisions. The least conducive arrangements are in highly majoritarian systems, such as two-party parliamentary systems. In such systems factions associated with the governing party are likely to prefer conflictual strategies. Political systems like that of the United States, with consensualism derived from institutional divisions, are the intermediate case. Under such institutions, competing factions may have little alternative to cooperation and yet may encounter major obstacles to negotiation.

Conclusion

First, I have stressed that many important policy conflicts are nonzero-sum; they present opportunity for cooperation and joint gain not only for a majority coalition but for all the groups or interests involved in debate. A crucial aspect of policy outcomes in such cases—as important as the relative success of the competing forces—is the degree to which they actually achieve cooperative outcomes. Second, I have proposed to analyze such issues as interactions—in effect, bargaining situations—between opposing issue factions. Taking into account the behavioral properties of such factions and the distinctive circumstances of policy conflict, I have used an informal game-theoretic analysis to define the general conditions that promote or inhibit cooperation. There are four general determinants of the factions' strategies: the joint gains available through cooperative policy making, the obstacles to reaching agreement, the potential losses in an unsuccessful negotiation, and the opportunity for distributive gains through conflictual means. Under certain circumstances, the factions have significant biases that also condition their response.

Third, I have applied these notions to consider how several elements of the policy process affect the prospects for cooperative outcomes: issue content, the structure of conflict, party politics, leadership, and political institutions. As the exercise demonstrates, if one takes the commonplace observation that political conflict is nonzero-sum seriously, it brings a new perspective to the study of policy making.

Notes

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1. That literature is immense. For game-theoretic and other rational choice analyses see Schelling (1980), Rapoport and Chammah 1965, Young 1975 and Hardin 1982; for social-psychological approaches, see Rubin and Brown 1975, Druckman 1977, and Pruitt 1982; in international relations, see Ikle 1964, Pillar 1983, Keohane 1984, and Oye 1986b; in labor relations, see Kochan 1980; for practical works on negotiation, see Fisher and Ury 1981, Raiffa 1982, and Zartman and Berman 1982.

2. In multifaction issues, certain groups, such as industries subject to import competition or those benefiting from tax preferences, may stand to lose from policies that serve collective interests. In part, outcomes will depend on the power of such groups in relation to those that stand to gain. For analyses emphasizing that aspect, see Baldwin 1985, Olson 1982, Pastor 1980 and 1983, and Witte 1985. But the outcomes also depend on whether many other groups subordinate parochial interests to advance collective ones, and whether the potential losers are offered and accept some accommodation (Goldstein 1986).

3. It is revealing, for example, that Harold Lasswell (1936) defined the central question of politics as "who gets what, when, and how," overlooking the question of what everyone gets; and that David Easton (1953) defined the function of the political system as "the authoritative allocation of values," implying a fixed supply of those values.

4. See, e.g., Riker 1962. For a nontechnical summary and recent bibliography, see Brams 1985. The coalition literature attempts to predict which subset of competitors will form a winning coalition and to estimate their relative power. Significantly, it is based on the theory of zero-sum games (Brams 1985).

5. The corporatism literature has had a restricted focus on the structure and development of corporatist institutions (Goldthorpe 1984). It neither compares corporatist polities' centralized negotiations with other processes of cooperation nor investigates determinants of cooperation other than structure.

6. For comparable uses of game theory in international politics, see Oye 1986b.

7. The effects of imperfect or asymmetrical information in relations between constituents and leaders have been studied from several perspectives. See Moe 1985, Rubin and Brown 1975, and Wilson 1973.

8. This tendency is partly an effect of divided and decentralized political structures and partly an effect of a cognitive bias that leads constituencies to react to losses more strongly than to gains (Kahneman and Tversky 1984; Weaver 1986).

9. The effectiveness of tit for tat depends on rather restrictive conditions concerning iterativeness, time preference, control, and recognition. See Oye 1986a.

10. All policy making involves extensive negotiation; this outcome refers to an ambitious negotiation with discussion of major concessions and effort to achieve joint gains.

11. I will argue, however, that the presence of numerous organized groups in a single issue faction affects perceptions and dispositions in a way that inhibits cooperation.

12. A variation on this argument—concerned with appointments to regulatory agencies—is developed in Moe 1987.

13. Indeed, in formal game theory, the analysis of nonzero-sum bargaining games generally stipulates that efficient agreements will be reached and goes on to examine criteria for dividing the benefits (Hamburger 1979).

14. This point resembles Axelrod's (1970) analysis of conflict of interest. Unlike Axelrod, however, I do not simply set these potential joint gains against the factions' divergent preferences and estimate their relative weight. Opposing issue factions by definition have conflicting interests. I assume that how aggressively they press those interests depends heavily on considerations of power and opportunity, and not simply on the divergence of preferences.

15. Both have been incorporated in issue typologies (see Lowi 1966; Wilson 1973), though with different analytic purposes.

16. On the contributions of mediators to cooperation, see Touval 1982.

17. Unlike individual incumbents, the parties are in zero-sum electoral conflict and cannot jointly benefit by sharing credit for policy change.

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ARMS CONTROL VERIFICATION AND OTHER GAMES INVOLVING IMPERFECT DETECTION

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This article presents an analysis of the strategic behavior of countries when there is imperfect verification of an arms control agreement. It provides a framework for determining whether an arms control agreement is desirable, shows which factors are needed for the agreement to be maintained in the absence of third-party enforcers, and develops propositions relating changes in verification capabilities to changes in the likelihood of cheating and the use of verification technology. These propositions yield several paradoxes of information (for example, the better the verification technology, the less often it will be employed). Since the analysis incorporates both simultaneous and sequential moves by the players, it provides new insights into other applied areas as well as game theory.

> No issue has loomed larger in Soviet-American arms control negotiations than verification.
>
> —Martin Einhorn, Gordon Kane, and Miroslav Nincic

> Verification stands as the litmus test by which arms control agreements are assessed.
>
> —William Potter

An arms control agreement involves the risk that one side's cheating may go undetected by the other side; a situation of no arms control involves a different type of risk. I provide a game theory analysis of how each side acts in the presence of such risks.

What issues determine whether an arms control agreement is possible? While much of the extensive literature on arms control suggests that the answer lies in the technological characteristics of verification, I argue that the technology of verification is just one part of a much more inclusive analysis that starts with the objective of each side. Clearly cheating on the arms control agreement is not the objective of the other side. Rather the likelihood of cheating depends (among other things) on the costs to the potential cheater of being caught, the benefits of not being caught, and the probability of being caught.

Even without the game theory analysis, important insights can be gained by putting the cost-benefit assumptions in a formal framework. By explicitly laying one's cards on the table, the limits of one's assumptions can be more readily understood than in a less formal analysis. For example, by exploring the possibility that the other side may want to exaggerate its offensive and defensive capabilities, I challenge the assumption implied in much published work that it tends to underreport its strength. Furthermore I show that it is not just a simple set of costs and benefits of cheating that must be considered but a matrix array (at least five

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payoff pairs). Hence, any empirical or intuitive discussion must discuss the whole range of issues presented here. For example, in discussing the feasibility of an arms control agreement it is necessary to estimate the cost to each side should one side incorrectly believe that the other side has cheated (an issue heretofore neglected).

The discussion of verification capabilities has often been the domain of natural scientists. Here I bring politics back into the analysis. I show that verification is not a fixed pair of probabilities—that of mistakenly believing the other side has been honest and that of mistakenly believing the other side has cheated (type 2 and type 1 errors)—but rather a continuum of probability pairs. Even if the verification technology cannot be altered to provide different type 1 and type 2 error combinations, the response to the output from the monitoring equipment can be. The detector (the country being cheated against) may choose different levels of evidence in which to respond. Thus the likelihood of the country making a type 1 or type 2 error is not merely a technological characteristic of the equipment but instead depends critically on the political response.

An important part of the cost-benefit calculations is the determination of the likelihood of cheating and the probability that cheating will be observed. But these probabilities are determined by the joint behavior of both sides: the likelihood of cheating depends on the probability of being observed, which depends on the likelihood of cheating. The game theory analysis allows us to disentangle these interwoven strategies.

Since an arms control agreement is not enforceable by a third party, one needs to ask why an agreement to go back to earlier levels would be self-enforcing if the previous level was not self-enforcing in the absence of an agreement. I provide the framework of analysis to answer this fundamental question.

More generally, I provide an analysis of behavior when information (regarding performance) is imperfect and both sides can act strategically. Hence, this theory may also be usefully applied to other areas, such as analyzing agreements between OPEC countries, determining the optimal punishment for crimes, and predicting strategic behavior between Congress and the presidency.

The analysis not only provides new insights into applied areas such as arms control but also yields new perspectives on game theory as theory. In the context of a game, if one side (the detector) monitors the other side's postrandomized choice, the game is turned from a simultaneous choice game to a sequential game, with the detector making its move after observing (perhaps imperfectly) the other player's move. The reward structure is considerably different in sequential and simultaneous games. I develop a game-theoretic model incorporating both sequential and simultaneous moves by the players.[1]

I introduce the concept of a simultaneous sequential game in the context of a zero-sum game. I demonstrate that if the detection equipment is less than perfect, the detector's optimal strategy may be to ignore the detection equipment.[2] I show that this strategy is equivalent to a simultaneous game where the detector sometimes chooses to act contrary to one of the signals. In turn, this is equivalent to the detector's trading off a higher rate of type 2 error for a lower rate of type 1 error. Then I model an arms control agreement as a nonconstant sum game. I give mathematical precision to some of the implicit assumptions made in the literature regarding the costs and benefits of arms control. I derive propositions relating the likelihood of cheating and the optimal use of the monitoring equipment to the accuracy of the monitor and show that the greater the accuracy of the monitor, the less likely it will be used. I also determine the condi-

Table 1. The Simultaneous-Sequential Game

Row	Simultaneous Game (C Does Not Look at Detector)		Sequential Game (C Looks at Detector)	
	C Assumes R Chose Row 1 (C_1, C_1)	C Assumes R Chose Row 2 (C_2, C_2)	Pure Strategy (C_1, C_2)	Mixed Strategy $(VC_1 + [1 - V]C_2, WC_2 + [1 - W]C_1)$
1	X_{11}	X_{12}	$PX_{11} + (1 - P)X_{12}$	$P[VX_{11} + (1 - V)X_{12}]$ $+ (1 - P)[WX_{12} + (1 - W)X_{11}]$
2	X_{21}	X_{22}	$QX_{22} + (1 - Q)X_{21}$	$Q[WX_{22} + (1 - W)X_{21}]$ $+ (1 - Q)[VX_{21} + (1 - V)X_{22}]$

Note: R maximizes. $X_{12}, X_{21} > X_{22}, X_{11}$. C minimizes.

tions for an arms control agreement to be feasible. Then I consider the conditions whereby an arms control agreement can produce an equilibrium even though the reduced level of armaments is not a Cournot equilibrium in the absence of an agreement. Essentially, the answer lies in the creation of a new matrix of outcomes generated by the agreement. In particular, I show the important role of verification. Finally, I extend the results to other areas including the punishment of criminals and the monitoring of agents.

Beyond these specific results, I create a framework of analysis for investigating arms control. I suggest some important issues to consider and appropriate questions to ask and delineate some key parameters that scientists of arms control should estimate and policy makers must determine. Thus the theory presented here creates a research agenda for future empirical work and a list of critical issues for future commentary on the topic of arms control.[3]

The Simultaneous-Sequential Methodology in the Context of Constant-Sum Detector Games

I present the simultaneous-sequential game, demonstrate that imperfect detection leads to mixed strategies, and show the equivalence between the simultaneous sequential approach and choosing a point on the type 1–type 2 trade-off curve. Because this section is devoted to constant-sum detector games, it is not appropriate for analyzing arms control, which is more accurately characterized as a non-constant-sum game. However, the analysis here serves as an important stepping-stone to the more complex non-constant-sum game and is valuable on its own terms for bringing insight to issues of detection in constant-sum games.

In order to make the logic as clear as possible I first consider the simultaneous game (no detection). The normal form representation of the simultaneous game comprises the first two columns in Table 1. I assume that R (row) maximizes, C (detector) minimizes, and $X_{12}, X_{21} > X_{11}, X_{22}$. Thus, no one row (or column) in the *simultaneous* game dominates another row (column).[4] The extensive form is considered in Appendix A.

We will now turn our attention to the sequential game. If R chooses R_i (possibly after a randomized draw from a mixed strategy between rows 1 and 2), there is some probability that C will detect that R has made this choice before C makes its choice (or while C can still alter its choice). Letting σ_j be the signal that R_j has been chosen, the probability of the monitoring equipment's signaling that R_j has been chosen when R has chosen R_i will be denoted by $\text{Prob}(\sigma_j | R_i)$. The probabilities of correct and incorrect detection given differing strategy choices (R_i) by R are then defined as follows:[5]

$$\text{Prob}(\sigma_1|R_1) = P;\ \text{Prob}(\sigma_2|R_1) = 1 - P;\ \text{Prob}(\sigma_2|R_2) = Q;\ \text{Prob}(\sigma_1|R_2) = 1 - Q.$$

I assume that $1 > P, Q > 1/2$. Since there are only two choices, the detector is better than a blind guess. It is possible that P (the probability of correct detection when row 1 is chosen) and Q (the probability of correct detection when row 2 is chosen) differ. For example, row 1 may be a maintenance in armaments, while row 2 may be an increase in the level of armaments. It may be hard to detect an increase in arms, in which case the detector has a high likelihood of mistakenly attributing row 1 (arms maintenance) when row 2 (arms increase) is true. However when row 1 (arms maintenance) is the case, attributing row 2 (arms increase) may be very unlikely.[6]

The sequential game, as the name implies, involves a sequence. R chooses a row (R_i). C is not able to observe this choice; however, the detection equipment emits a noisy signal (σ_1 or σ_2), which C is able to observe. C then chooses a strategy, C_j (in the sequential game the C_j are conditional on the observation of the signal and stand for the columns in the simultaneous game). The resulting outcomes are denoted by X_{ij}.

We are now ready to incorporate the possibility of detection (with error) into an expanded game matrix (cols. 3 and 4 in Table 1). Column 3 represents payoffs of the sequential game when C (the detector) undertakes the following pure strategy: if the signal indicates that R_1 has been chosen (that is, σ_1 is observed), C chooses C_1; if the signal indicates that R_2 has been chosen, C chooses C_2. At the top of column 3 the vector C_1, C_2 represents C's response to signals σ_1 and σ_2, respectively.[7] It should be noted that C_i is C's best response to R_i since $X_{12}, X_{21} > X_{22}, X_{11}$.[8] When R chooses R_1, $100P\%$ of the time the detector will yield the correct signal, σ_1, that R has indeed chosen R_1. If C chooses C_i in response to σ_i, $100P\%$ of the time the outcome will be X_{11}, and $100(1 - P)\%$ of the time the outcome will be X_{12}. Hence, the outcome is that represented in the first row of column 3. A similar analysis will yield the second row of column 3. Note that column 3 involves sequential moves by C after R has made its postrandomized choice but that the decision whether to use column 3 in the first place is made simultaneously with—or in the absence of information about—R's decision. Thus, the decision to act sequentially is made simultaneously. The first two columns in Table 1 represent the simultaneous game, while the third column represents the sequential game. Hence, the first 3 columns represent a simultaneous-sequential game.

Column 4 represents the outcome of the sequential game when C (the detector) may act contrary to the detector by undertaking a mixed strategy in response to a signal. V is the probability of C's choosing C_1 and $1 - V$ is the probability of C's choosing C_2 when σ_1 is observed; while W is the probability of C's choosing C_2 and $1 - W$ is the probability of C's choosing C_1 when σ_2 is observed. Looking at row 1, column 4, when R chooses R_1, C will observe σ_1 with probability P; and given that σ_1 has been observed, C will choose C_1 (yielding outcome X_{11}) with probability V and will choose C_2 (yielding outcome X_{12}) with probability $1 - V$. There is also a probability of $1 - P$ that C will observe σ_2 when R chooses R_1. Given that σ_2 has been observed, the outcome will be X_{12} with probability W and X_{11} with probability $1 - W$. At the top of column 4, the vector $VC_1 + (1 - V)C_2, WC_2 + (1 - W)C_1$ represents C's response to signals σ_1 and σ_2, respectively.

Each of the first three columns in Table

1 are special cases of column 4. If $V, W = 1$, column 4 is equivalent to column 3; if $V = 0$ and $W = 1$, column 4 is equivalent to column 2; and if $V = 1$ and $W = 0$, column 4 is equivalent to column 1. Thus, column 4, the sequential game where C may act contrary to the signal, spans the same space as columns 1, 2 and 3, the simultaneous-sequential game (the simultaneous part is that C may ignore the signal entirely; the sequential part that if C makes use of the signal, it will never act contrary).

PROPOSITION 1. *C's optimal strategy is a mixed strategy.*

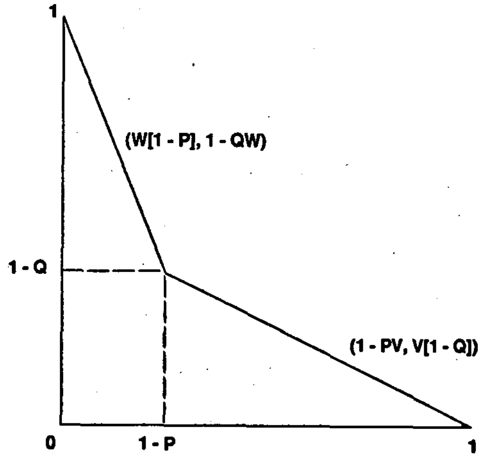
Proof. According to Nash (1951) an equilibrium exists. If either $0 < V < 1$ or $0 < W < 1$, column 4 is a mixed strategy. Note that in columns 1, 2, and 3 and rows 1 and 2 no pure strategy equilibrium exists. Therefore, there must be a mixed strategy equilibrium. Thus, if column 3 is used (as part of C's mixed strategy), it must be used with probability less than 1; and if column 4 is used, column 4 itself involves mixed strategies. QED.

PROPOSITION 2. *A mixed strategy between columns 1 and 3 yields an equivalent outcome to the mixed strategy within column 4 (when $V = 1$); while a mixed strategy between columns 2 and 3 yields an equivalent outcome to the mixed strategy within column 4 (when $W = 1$). These mixed strategies are superior to any other mixed strategy. The optimal strategy by row also involves a mixed strategy.*

The proof is in Appendix B. Here I will provide the intuition. Proposition 2 states that an optimal strategy by C requires that some percentage of the time, either C acts contrary to one (and only one) of the signals emanating from its detection equipment or (equivalently) C ignores its detection equipment entirely (and acts without any knowledge of the signal).[9] The intuition for the equivalence result can be established via an example. Looking at Table 1, consider the case where C chooses column 1 with probability .1 and column 3 with probability .9. If R chooses R_1, X_{11} will occur with probability $.1 + .9P$; if R chooses R_2, X_{22} will occur with probability $.9Q$. I will now compare this to the result where C chooses column 4, $V = 1$, and $W = .9$. Looking at column 4, if R chooses R_1, X_{11} will occur with probability $P + (1 - P)(1 - .9) = .1 + .9P$; if R chooses R_2, X_{22} will occur with probability $.9Q$. Hence, both methods lead to the same result. The decision to not use the monitor has nothing to do with its being costly to use in the conventional sense, for I have not assumed that there is a cost (in man-hours, for example) to using it.

The idea of a mixed strategy equilibrium may be a hard pill to swallow. However, I will now show that it is equivalent to a pure strategy equilibrium when the monitoring technology is allowed to vary in a specified way. I have characterized the error possibilities as being $(1 - P)$ and $(1 - Q)$. These can be seen as parameters for a family of type 1 and type 2 errors, respectively. For example, consider a smoke alarm, where smoke is row 2 and no smoke is row 1. The probability of the alarm going off when there is no smoke (type 1 error) is $1 - P$; while the probability of the alarm not going off when there is smoke (type 2 error) is $1 - Q$. A smoke alarm can be adjusted so that it is less sensitive to particles in the air. In this way, the probability of a type 1 error (the likelihood of the alarm going off when there is no fire) is reduced. However, this adjustment will increase the probability of a type 2 error. Hence an adjustable smoke alarm is a trade-off curve between type 1 and type 2 errors rather than a point $(1 - P, 1 - Q)$. Figure 1 is a kinked linear type 1-type 2 error trade-off curve, the set of points being $(W[1 - P], 1 - QW)$ for $0 \le W \le 1$ and $(1 - PV, V[1 - Q])$ for 0

Figure 1. The Type 1–Type 2 Error Trade-off Curve



Note: The vertical axis is the probability of a type 2 error; the horizontal axis is the probability of a type 1 error.

$\leq V \leq 1$. If $W = 1$, the probability of type 1 error (given that R has chosen r_1) is $1 - P$, while the probability of a type 2 error (given that R has chosen r_2) is $1 - Q$; that is, we are at the kink. If $W = 0$ (that is, C chooses a monitoring technology that always calls for C_1 to be chosen), the probability of a type 1 error is zero and the probability of a type 2 error (given R_2) is 1 (upper left-hand corner of the graph). If the trade-off curve is the one depicted in Figure 1, we have the same set of options as in column 4, Table 2 (for $V = 1$, the probability of a type 1 error, X_{12}, is $W[1 - P]$ and the probability of a type 2 error, X_{21}, is $Q[1 - W] + 1 - Q = 1 - QW$). However, W is no longer a randomized strategy but instead represents a particular technology choice along the type 1–type 2 trade-off curve.[10]

I have considered three alternative viewpoints that on closer inspection are equivalent. These are (1) ignoring the detection equipment, (2) going contrary to the detection equipment, and (3) choosing a particular point on the type 1–type 2 (kinked line) trade-off curve. I will now concentrate on the simultaneous-sequential approach where C may at times make choices in the absence of knowledge concerning the detector. While my analysis will be in terms of "ignoring the detector," the reader may choose to substitute "go contrary to the detector" or "choose a higher rate of type 2 error."

The Matrix of Outcomes in Non-constant-Sum Arms Control Detector Game

The simultaneous-sequential framework can be used to analyze any game. I concentrate on arms control. It is not easy to model arms control as a game. The ordinal relationships I posit are reasonable, but arguments can be made for other rankings. Consider the expressed fear that the other side may cheat in the presence of an arms control agreement. Each side may try to convince the other that it has reduced its weaponry when it has in fact not done so. In particular, a side that intends to attack will underreport its armaments, hoping to lull the other side into a false sense of security. However, arguments can be made for lying in the other direction as well. A country that fears attack will exaggerate its armaments, thereby hoping to dissuade the other side from attacking.[11] Exaggeration may also help an aggressor country get its way without resorting to war. I will rule out exaggeration because underreporting of armaments is the perceived concern in arms control. In particular, if there is no arms control agreement, I assume that neither R nor C has surreptitiously reduced its armaments.

The players have two games: (1) no arms control and (2) arms control, encompassing a possible return to a situa-

Table 2. The Arms Control Game

	Simultaneous Game (C Does Not Look at Detector)		Sequential Game (C Looks at Detector)	
Arms Control Game	C Assumes R Chose Row 1 (C_1, C_1)	C Assumes R Chose Row 2 (C_2, C_2)	Pure Strategy (C_1, C_2)	Mixed Strategy $(VC_1 + [1 - V]C_2, WC_2 + [1 - W]C_1)$
Row 1: Lives up to Arms Control Agreement	X_{11}, Y_{11}	$X_{\bar{3}\bar{3}}, Y_{\bar{3}\bar{3}}$	$PX_{11} + (1 - P)X_{\bar{3}\bar{3}},$ $Py_{11} + (1 - P)Y_{\bar{3}\bar{3}}$	$P[VX_{11} + (1 - V)X_{\bar{3}\bar{3}}] + (1 - P)[WX_{\bar{3}\bar{3}} + (1 - W)X_{11}],$ $P[VY_{11} + (1 - V)Y_{\bar{3}\bar{3}}] + (1 - P)[WY_{33} + (1 - W)Y_{11}]$
Row 2: Cheats on Arms Control Agreement	X_{21}, Y_{21}	$X_{\bar{3}\bar{3}}, Y_{\bar{3}\bar{3}}$	$PX_{33} + (1 - P)X_{21},$ $PY_{\bar{3}\bar{3}} + (1 - P)Y_{21}$	$P[VX_{\bar{3}3} + (1 - V)X_{21}] + (1 - P)[WX_{21} + (1 - W)X_{\bar{3}3}],$ $P[VY_{\bar{3}\bar{3}} + (1 - V)Y_{21}] + (1 - P)[WY_{21} + (1 - W)Y_{\bar{3}\bar{3}}]$

Note: $X_{21} > X_{11} > X_{33} > X_{\bar{3}3} > X_{\bar{3}\bar{3}}$; $Y_{11} > Y_{33} > Y_{\bar{3}\bar{3}} > Y_{21}$.

tion of no arms control (see Tables 2 and 3). If *R* announces that it has not reduced its armaments (that is, there is no arms control agreement), *C* knows that the no-arms-control-agreement game is being played. If *R* announces that it has reduced armaments, *C* knows that *R* has chosen either row 1 or row 2 in the arms control game. The arms control game is played only if both sides have a higher expected payoff under the arms control game than that under a situation of no arms control. Within the arms control game, *R* has two pure strategies available, and *C* has three (columns 1, 2 and 3).

To simplify, I assume that *C* is honest with regard to arms control (perhaps *R* has a perfect detector, and any violation of the agreement by *C* will be detected). Thus, *C*'s decision regarding arms control is implicitly built into both matrices (see Table 2). If *R* has not agreed to arms control, neither has *C*, and the outcome is X_{33}, Y_{33} in the no-arms-control game matrix. If *R* has agreed to arms control, *C* has also agreed to arms control, and there is no cheating on the agreement by *C* in the arms control game matrix. In Appendix C the completely symmetrical case,

Table 3. No Arms Control Game

No Arms Control Game	C
R	X_{33}, Y_{33}

with both sides detecting and having the option of cheating, is investigated.

I assume that in the arms control game if C chooses C_2 (C "believes" that R has cheated), the situation reverts to that of no arms control.[12] To simplify the discussion I assume that $P = Q$.

Looking mainly at the first two columns (the simultaneous part) of the arms control game in Table 2, I make the following additional assumptions (concentrating at first mainly on R).

1. $X_{11} > X_{33}$ and $Y_{11} > Y_{33}$; (1)

   that is, there exists some arms reduction agreement that if fully enforced and maintained, would make both sides better off.[13]

2. $X_{21} > X_{11}$. R prefers X_{21} (not reducing arms at all or not to the level stipulated in the agreement but having C believe it has reduced arms to the stipulated level) over X_{11} (reducing armaments and having C believe it). I make this assumption since this is the perceived problem in arms control—that the country gains by cheating on the agreement if it can avoid detection.[14]

3. $X_{\bar{3}3} < X_{33}$. When C detects a violation before R can benefit from it (by employing strategy C_1, C_2 of the sequential game and observing σ_2) or acts as if a violation has occurred (by employing strategy C_2, C_2 in the simultaneous game), the outcome is very similar to the outcome when there is no arms control agreement, X_{33}, except for an initial difference.[15] For example, if arms control means destruction of easily detected armaments by R but R cheats and fails to reduce arms that cannot be easily detected, going back to a situation of no arms control implies that R will be replacing the recently destroyed arms. Under this characterization R's cheating and being caught before it can gain any benefit would be worse for R than never having had arms control in the first place, although for the most part the outcome would be similar to the outcome if there had never been arms control. Therefore, to characterize this stream of events I will use the notation $X_{\bar{3}3}$ ($< X_{33}$).

4. $X_{\bar{3}3} > X_{\bar{3}\bar{3}}$. If R has indeed lived up to the arms control agreement but C mistakenly believes that R has violated the agreement, once again the outcome is basically similar to that of no arms control. However, the difference in level of armaments between the two states of the world is greater than if R had actually cheated and been caught. Therefore, I connote the outcome by $X_{\bar{3}\bar{3}}$ ($< X_{\bar{3}3}$).[16]

5. $0 < Y_{21} < Y_{ij}$ (for $ij \neq 21$). This concerns C's preferences. The worst outcome for C is to believe that (or act as if) R has lived up to an arms control agreement when R has actually cheated on the agreement. Once again (but now looking from the viewpoint of C), this is the perceived fear.[17]

6. $Y_{33} > Y_{\bar{3}\bar{3}}$. C prefers no arms control over the following stream of events: an initial arms control agreement, believing that R has cheated, and a resulting dissolution of the agreement. The line of reasoning follows that for R. Thus, for example, destruction and replacement of arms is more costly to C than is not destroying them in the first place.[18]

The arms control game has four columns. Columns 1 and 2 are the simultaneous game; columns 3 and 4 are the sequential game. Column 4 is the mixed strategy response to signals σ_i. In column 3, $V = W = 1$; hence, column 3 is a pure strategy response to signals σ_i and a special case of column 4.[19]

Equilibrium in Arms Control Detector Games

The Optimal Strategy

I now determine each side's optimal strategy under an arms control agreement. I will forego the tedious proof that column 4 (with $V = 1$) is equivalent to a mixed strategy between columns 1 and 3; and instead ignore column 4 in the analysis. The reader is free to substitute the words "*C* acts contrary to the signal that *R* has cheated" or "*C* chooses a higher type 1 error rate" for the phrase "*C* acts in the absence of knowledge regarding the detection equipment and assumes that *R* has not cheated."

In the following proposition I show that any arms control agreement involves some chance that *R* will cheat and that *C* will choose to increase the probability of a type 1 error by at times ignoring evidence of cheating by *R*. I also show that an arms control agreement is impossible if *R* can improve its welfare by cheating when *C* is using the monitor.

PROPOSITION 3. *If the detection equipment is less than perfect ($1/2 < P = Q < 1$), it holds that (1) the arms control agreement game has a unique pure strategy equilibrium, and R and C prefer the no-arms-control outcome over this pure strategy equilibrium; (2) there exists a mixed strategy equilibrium under an arms control agreement if and only if $P(X_{11}) + (1 - P)X_{\bar{3}\bar{3}} > P(X_{\bar{3}3}) + (1 - P)X_{21}$, that is, when C uses the detector, R's expected utility is higher when R is honest; (3) C prefers no arms control to any equilibrium that includes (C_2, C_2).*

Proof of Part 1. Row 2, column 2 is a pure strategy equilibrium. If *R* chooses row 2, *C*'s best strategy is to choose column 2 since $Y_{\bar{3}\bar{3}} > Y_{21}$ by assumption. If *C* chooses column 2, *R*'s best strategy is to choose row 2, since $X_{\bar{3}3} > X_{\bar{3}\bar{3}}$. $X_{\bar{3}3} < X_{33}$ and $Y_{\bar{3}\bar{3}} < Y_{33}$. Therefore, both *R* and *C* would prefer no arms control agreement to such a pure strategy equilibrium. It is quite easy to establish that no other pure strategy Nash equilibrium exists. The only other pure strategy available to *R* is to choose row 1. *C*'s best choice, given row 1, is to choose column 1; but *R*'s best choice, given column 1, is to choose row 2. Thus no other pure strategy equilibrium exists. Since *R*, as well as *C*, prefers no arms control to the outcome of an arms control equilibrium in pure strategies, if an arms control agreement exists, a mixed strategy equilibrium must be possible.[20]

Proof of Part 2.[21] A necessary condition for a mixed strategy equilibrium is that row 2 does not dominate row 1. Since $X_{21} > X_{11}$ and $X_{\bar{3}3} > X_{\bar{3}\bar{3}}$, a necessary condition for a randomized equilibrium is that the reverse relationship holds for column 3: $P(X_{11}) + (1 - P)X_{\bar{3}\bar{3}} > P(X_{\bar{3}3}) + (1 - P)X_{21}$; that is, when *C* uses (only) the imperfect monitor (column 3), the expected utility to *R* is greater when *R* is honest than when it cheats on the arms control agreement. From now on I will assume that these necessary and sufficient conditions exist. Note that for $P = .5$ these conditions are not satisfied and that as *P* increases, these conditions are more likely to be satisfied. Thus the quality of the monitoring may play an important role in the feasibility of an arms control agreement.[22]

Proof of Part 3. I will provide the underlying intuition for the proof (see Wittman 1987 for the details). In a mixed strategy equilibrium, randomization by *R* makes *C* indifferent between the two columns it has chosen for its mixed strategy. If column 2 is one of *C*'s choices, *C* can expect $Y_{\bar{3}\bar{3}}$, which is less than the outcome if there is no arms control agreement (Y_{33}). Therefore, *Y* would not enter into this agreement in the first place. QED

Thus, if there is an arms control agreement, it involves a mixed strategy equilibrium with C sometimes not observing its monitoring equipment and assuming that R has been honest (C is choosing a greater rate of type 1 errors). Proposition 3 may provide an explanation for why one side may choose to ignore information that appears to show that the other side has violated the agreement.

The Effect of Monitoring Quality on *C* and *R*'s Behavior

So far the focus has been on the existence or nonexistence of particular pure and mixed strategy equilibria. We now turn our attention towards comparative statics in order to discover the relationships between the quality of the monitoring device, the costs and benefits to the players, and the equilibrium strategies.

$$\text{Let } K \equiv (Y_{\overline{33}} - Y_{21})/(Y_{11} - Y_{\overline{33}}). \quad (2)$$

Note that in the first two columns in Table 2, K is the ratio of C's loss from mistakenly believing that R has been honest to C's loss from mistakenly believing that R has been dishonest. With honesty by R as the null hypothesis, K is the ratio of the loss from a type 2 error to that from a type 1 error.

Let c_1 stand for the probability of C's choosing column 1 and r_1 stand for the probability of R's choosing row 1.

PROPOSITION 4. *(1a) As K increases, so does r_1 (the probability of R's choosing row 1, not cheating); (b) As $K \rightarrow 0$, $r_1 \rightarrow 0$; (c) At $K = 1$, $r_1 = P$ (the probability that the detector is correct); (d) As $K \rightarrow \infty$, $r_1 \rightarrow 1$; (2) As P increases, so does r_1.*

The proof is in Appendix D. Here I will provide the explanation and the intuition.

I first consider part 1. At a Cournot-Nash equilibrium, C's optimizing strategy makes R indifferent between the rows, and R's optimizing strategy makes C indifferent between the columns. C is choosing between columns 1 and 3 and in equilibrium is indifferent between them (see Table 2). If Y_{21} decreases (thereby increasing K) and R does not change its equilibrium strategy, C will prefer column 3 (since C's expected utility is reduced only by $r_2(1 - P)$ times the change in Y_{21} when C chooses column 3 instead of being reduced by r_2-times-the-change-in-Y_{21} when C chooses column 1). Therefore, in order to make C once again indifferent, R increases its probability of choosing row 1, thereby increasing C's expected utility from choosing column 1 (relative to column 3). Hence, in equilibrium, R acts like an altruist—as the relative cost to C from R's cheating increases (i.e., K increases), the probability of R's cheating decreases in order to maintain C's indifference as to the choice of column.

When $K < 1$ (that is, when the cost to C of mistakenly believing that R has been honest is relatively low), the probability of R's cheating $(1 - r_1)$ will be greater than the probability of C's monitor being incorrect $(1 - P)$; and when K is greater than 1, the probability of R's being honest (r_1) will be greater than the probability of C's monitor being correct (P).[23] When Y_{33} is approximately equal to $Y_{\overline{33}}$, an approximate rule of thumb is that $r_1 > P$ if the outcome under a situation of no arms control is closer to that when R is honest and C believes R has been honest than to that when R cheats on an arms control agreement contrary to C's belief.

Now I turn to part 2. When the quality of C's detector improves, R will be more likely to be honest. The logic is best understood by again starting with the equilibrium outcome where C is initially indifferent between columns 1 and 3. When the quality of the detector improves, the expected payoff to C from using the detector (column 3) increases. If R did not alter its strategy, C would prefer column 3. Therefore, R makes C indifferent by increasing the probability of R's not cheating (that is, increasing r_1).

I next consider the relationship between P and c_1.

PROPOSITION 5. *As P (the quality of the monitor) increases, $1 - c_1$ (the likelihood of C's using the monitor) decreases; (2) The smaller X_{33} and $X_{\overline{33}}$ are, the less likely C will use its detector and the more likely it will assume that R has been honest, that is, when a termination of the arms control agreement significantly reduces R's utility, C is more likely to assume that R has been honest—c_1 increases; (3) The greater the payoff to R from cheating and not getting caught, the greater the probability that C will use its monitor; that is, as X_{21} increases, c_1 decreases.*

The proofs are in Appendix E. Part 1 states that the better the detection equipment, the less likely the detector will use it and the more likely the detector will implicitly assume that the other side is honest by undertaking strategy C_1, C_1. The explanation for this counterintuitive result is that when C's detection equipment improves, R's optimal strategy is to be honest more often (as demonstrated in proposition 4); thus C can partially take advantage of this increased honesty by assuming that R has been honest.

The game theory logic for part 2 is that if R has more to lose from a failure of the arms control agreement, C makes R indifferent between the two strategies by assuming more often that R is honest. For part 3, a gain to R from cheating and getting away with it results in C's lowering the probability of R's assuming that C has not cheated.

The Conditions for an Arms Control Agreement To Be Possible

Now I turn to the most critical issue: Is an arms control agreement possible? A necessary condition for an arms control agreement is that both sides be better off than under no agreement.[24] The conditions for an arms control agreement yielding greater expected utility to R than no arms control are contained in the following proposition.

PROPOSITION 6. *If $PX_{11} + (1 - P)X_{\overline{33}} > X_{33}$, R prefers the arms control equilibrium to the outcome when there is no arms control.*

The proof is in Appendix F. Since I have assumed that $P > 1/2$ and that $X_{\overline{33}}$ is relatively close to X_{33}, this condition is likely to be met.

Not surprisingly, the conditions for C's entering into an arms control agreement are more complex, as can be seen in proposition 7.

PROPOSITION 7. *For an arms control equilibrium to be preferred by C to the equilibrium under no arms control, the following inequality must hold:*

$$\frac{Y_{\overline{33}}(KP + P) + (1 - 2P)Y_{21}}{1 - P + PK} \geq Y_{33}.$$

The proof is found in Appendix G. In order to get a feel for the conditions implied by this inequality I will consider some polar cases. It is easy to establish that an arms control agreement is impossible when the monitoring equipment is no better than a blind guess (i.e., $P = 1/2$). At $P = 1/2$, the left side of the inequality in proposition 7 reduces to $Y_{\overline{33}}$. $Y_{\overline{33}}$ is less than Y_{33} by assumption. At $P = 1$, an arms control agreement is always possible. Letting $P = 1$ and making use of the definition for K (equation 2), the left side of the inequality reduces to

$$Y_{\overline{33}} + \frac{Y_{\overline{33}} - Y_{21}}{K} = Y_{\overline{33}}$$

$$+ (Y_{\overline{33}} - Y_{21})\left(\frac{Y_{11} - Y_{\overline{33}}}{Y_{\overline{33}} - Y_{21}}\right)$$

$$= Y_{11} > Y_{33},$$

by assumption.

Finally, at

$$K \equiv \frac{Y_{\bar{3}\bar{3}} - Y_{21}}{Y_{11} - Y_{\bar{3}\bar{3}}} = 1,$$

the left side of the inequality reduces to

$$Y_{\bar{3}\bar{3}} + (2P - 1)(Y_{\bar{3}\bar{3}} - Y_{21}) = Y_{\bar{3}\bar{3}} + (2P - 1)(Y_{11} - Y_{\bar{3}\bar{3}}).$$

Thus, for a $P = .75$, any differential between Y_{11} and $Y_{\bar{3}\bar{3}}$ greater than twice the differential between Y_{33} and $Y_{\bar{3}\bar{3}}$ will mean that *C*'s expected utility under an arms control agreement equilibrium is greater than *C*'s expected utility when there is no arms control.

The inequality relationship in proposition 7 may also provide insight into the effect of increased armament reduction. With fewer armaments remaining, Y_{21} (the cost to *C* of mistakenly believing that *R* has been honest) is likely to increase. In turn, this reduces the left-hand side of the inequality in proposition 7 as Y_{21} is multiplied by $(1 - 2P)$, a negative number. Hence the need for improved verification. Yet this is not the complete story as the left-hand side of the inequality also suggests that we must consider other variables, such as the costs of a failed disarmament treaty, in deciding whether an arms control agreement is possible.

It has been demonstrated that any arms control agreement in which detection is less than perfect involves some strictly positive probability of *R*'s cheating and some strictly positive probability of *C*'s not paying attention to its detection equipment (C_1, C_1 is chosen with probability greater than 0). Even though the probability of undetected cheating's occurring is greater than zero, an arms control agreement is appropriate for *R* and *C* if the expected utility is greater than under a situation of no arms control. The possibility of undetected cheating is not a sufficient reason for not having an arms control agreement. As in other areas, the expected costs and benefits must be weighed. To quote President Kennedy's message to the Senate (1963) regarding a nuclear test ban: "The risks of detection outweigh the potential gains from violation, and the risk to the United States from such violation is outweighed by the risk of a continued unlimited nuclear arms race."[25] An example of this thinking can be found in the U.S.-Soviet treaty prohibiting nuclear underground tests above 150 kilotons. It is impossible to distinguish between 153 kilotons and 147, but such minor cheating would have little effect on the balance of power.[26]

Self-enforcing Agreements

An arms control agreement between two superpowers is not enforced by a third party. Why, then, should there be an arms control agreement equilibrium if there is no equilibrium at that level of armaments in the absence of an arms control agreement? To put the problem in concrete terms, If an arms control agreement puts both parties back to the level of armaments that each had two years previously, what forces exist now (which did not exist previously) to maintain the equilibrium level of armaments at the two-year-old level? Here I will provide some of the possible explanations.

The arms control equilibrium may be unstable, or one of many possible equilibria. Thus, if newly developed monitoring capabilities or new information about the greater costs of war create a new, low level-of-armaments equilibrium, simple Cournot behavior may not lead to it. Simultaneous behavior (i.e., non-Cournot behavior) by both parties by means of an explicit arms control agreement may be necessary to get back to the equilibrium encompassing low levels of expenditures on armaments.[27]

The arms control agreement itself may change the payoff matrix, thereby creating equilibria that were not possible in the absence of an agreement. Thus, by agreement, each side may allow the other side to have on-sight surveillance (thereby increasing P). As shown earlier, the probability of detecting increased armaments must be of a certain size before arms control is better than no arms control. Both sides may require improved monitoring, and without simultaneous improvements in monitoring the equilibrium may not be achieved by Cournot behavior. Thus, once again, verification plays an important role. The recent International Nuclear Forces treaty may have been implemented because of the increased Soviet willingness for on-sight inspection.

The agreement may also involve the simultaneous exchange of "hostages."[28] Providing hostages without a *quid pro quo* will typically hurt the hostage-giving country. So Cournot behavior would not result in an exchange of hostages. Only an agreement involving the simultaneous exchange of hostages by both parties may be possible. The matrix of payoffs is then changed (e.g., X_{21} is reduced), thereby reducing the benefit of increased arms.

Extensions to Other Areas

The framework developed here can be applied to other areas, such as the punishment of criminals and the accountability of politicians. The main alteration is to the elements of the two-by-four matrix. Clearly, different objective functions and different levels of detection are likely to alter the payoff structures and the optimal strategies by the players. In particular, for some applications, pure strategies might dominate mixed strategies.

It is insightful to contrast the approach used in this paper with previous work outside of the arms control literature. I first consider research concerned with the optimal punishment of criminals. Becker (1968) and, more recently, Polinsky and Shavell (1979) consider the possibility that not all criminals are detected and convicted; but they have not considered, as I have here, the other type of error—that innocent people may be convicted. However, from society's (the detector's) view, the possibility of punishing an innocent person should be a very important consideration.

In principal-agent relationships there may be imperfect monitoring. An incumbent (agent) who voters mistakenly believe has shirked (not acted in the interests of the majority) may lose an election, and a shirking incumbent who voters mistakenly believe has implemented the interests of the majority may be reelected. The principal-agent literature assumes that the principal always acts according to its monitoring equipment. It does not consider the possibility that the principal might choose to ignore the information, nor does it consider the role of type 1 and type 2 errors on the principal's or the agent's optimal strategies.[29]

As another example, consider OPEC agreements to limit production. There is considerable potential for undetected cheating. The failure to hold prices in line suggests that quotas are not being rigidly adhered to. If there is imperfect monitoring, verification of OPEC agreements has obvious similarities to the verification problem in arms control.

From a more general perspective, theorists have developed two separate models of behavior, one involving simultaneous play games and the other involving sequential play. As shown here, imperfect monitoring (in comparison to no monitoring) alters the simultaneous game into a hybrid simultaneous-sequential game; similarly, imperfect monitoring (in comparison to perfect monitoring) by the second player in a sequential game alters the pure sequential game into a hybrid simultaneous-sequential one. The methodology used here integrates these two separate strands into a more general approach where one or both players choose (if both, simultaneously) whether to play the simultaneous or the sequential game.

Conclusion

All actions, including arms control agreements, involve some risk. I have argued that requiring a risk-free arms control agreement is to make the wrong calculations.[30] Rather, one must compare expected utilities with and without arms control. I have shown how to make the appropriate calculations by developing a game theory model incorporating both simultaneous and sequential behavior. The monitored country (*R*) knows that the monitoring country (*C*) may detect the monitored country's choice, albeit imperfectly. The strategy of each country takes this possibility into account as well as the knowledge that each side can replicate the other's thinking. I derive results concerning optimal levels of verification and cheating by each country and demonstrate the inherent randomness of optimal strategic behavior.

I have provided two paradoxes of information that may occur when the evidence is imperfect and both sides engage in strategic behavior: (1) a player should purposely ignore the evidence on occasion, even though the evidence is more likely to be correct than incorrect, and (2) the better the information, the more the player should ignore it. For arms control in particular, this latter paradox implies that improvements in verification technology will lead to a greater willingness to assume that the other side has not cheated. Intuitively, the paradox is partially resolved in light of the realization that the objective of the country is not to maximize the probability of making the correct choice but rather to maximize its expected utility.

Since an arms control agreement is not enforceable by a third party, why would an agreement to go back to earlier levels be self-enforcing if the previous level was not self-enforcing in the absence of an agreement? The explanation is not found in a change in attitude but rather in a change in the payoff matrix. In particular, improved verification capabilities created by the agreement (such as on-sight inspections) may significantly alter the incentives to cheat. In contrast, partial unilateral disarmament adjustments may take place in the absence of an agreement if there is an improvement in monitoring technology not requiring joint action by the countries.

In calculating each country's expected payoff from the arms control game a matrix array of costs and benefits must be determined. These include the cost to country *C* and the benefit to country *R* when *R* cheats (both with and without detection), the cost to each country when one side is falsely accused of cheating, and the benefit to each country when there is no cheating (and there is no accusation of cheating), as well as the probabilities of failing to detect cheating and mistakenly believing that cheating has occurred. These costs, benefits, and probabilities must be considered even by those who undertake a less formal analysis.

Although a matrix of cost-benefit calculations is more complex than a simple pair, this does not mean that empirical hypotheses are impossible. In generating empirical predictions, one can often rely on *ceteris paribus* clauses. For example, other things being equal, the less benefit that a country gains from cheating and the more accurate the monitor, the greater the likelihood that the particular weapon will be subject to an agreement. Since different weapon systems vary in their ability to be detected and their effect on each country's expected payoff, there is a great potential to test the theoretical relationships developed here.

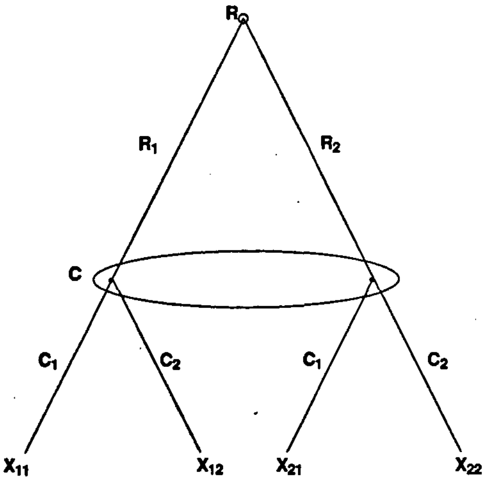
Although the probability of the monitoring equipment's giving a false signal is exogenous, the actual error rate is shown to be endogenous. The detector can improve its welfare by intentionally ignoring the equipment. Equivalently, the detector has a choice of which point on the type 1–type 2 trade-off curve it will choose. To choose the monitoring technology that minimizes type 2 error would itself be in error, since the appropriate monitoring technology maximizes expected utility of the detector. Indeed, by observing the chosen technology with its type 1 and type 2 errors, future researchers may be able to infer something about the utility functions.

The endogeneity of behavior has yielded numerous qualitative relationships. For example, improvement in monitoring will result in a monitored country's being honest more often and the monitoring country's choosing a higher rate of type 2 error (assuming the other side is honest when it is not). This latter proposition may also be tested.

Other interesting extensions and refinements of this argument are possible. For example, the probability of detection and the cost to country *C* of cheating by country *R* may be a function of time or of the degree of cheating by *R*. Also, the level of armaments may be known within a range rather than in terms of an honest-dishonest dichotomy. As a final example, the analysis of type 1 and type 2 errors can be extended to analyze the symmetric case. All of these issues can be incorporated into the basic framework provided here.

The large literature on arms control verification has focused on the technical aspects of monitoring. I have treated the monitoring capabilities as given and instead concentrated on the optimal strategy, given certain levels of risk. I have thus turned what appears to be merely a technical problem into a question of political choice.

Figure A-1. The Extensive Form of the Simultaneous Game



Appendix A: The Game in Extensive Form

In Figure A-1, I have drawn an extensive form version of the simultaneous game. Starting at the root of the tree, *R* chooses a row; *C* chooses a column without knowledge of *R*'s choice (hence the balloon around both of *R*'s choices, indicating that *C*'s information set cannot distinguish between the two); at the end of the game tree the X_{ij} outcomes (when *R* chooses *i* and *C* chooses *j*) are listed.

I next consider the extensive form for the sequential game. Note that in Figure A-2, *R* chooses a row, *C* is not able to observe this choice, a noisy signal (σ_i) is emitted, *C* observes this signal, and *C* chooses a strategy, C_i (in the sequential game the C_i are conditional on the observation of the signal and stand for the columns in the simultaneous game). The resulting outcomes are denoted by X_{ij}.

The extensive form game presented in Figure A-2 is only a partial characterization of this simultaneous-sequential game. The complete characterization involves both Figures A-1 and A-2 and is drawn in

Figure A-2. The Extensive Form of a Sequential Game

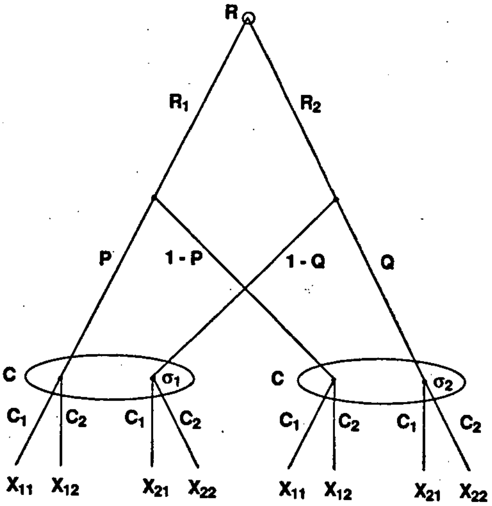

R
R_1
R_2
P
1 - P
1 - Q
Q
C
σ_1
C
σ_2
C_1 C_2 C_1 C_2
C_1 C_2 C_1 C_2
X_{11} X_{12} X_{21} X_{22}
X_{11} X_{12} X_{21} X_{22}

Figure A-3. Essentially, C has the option of choosing the sequential or the simultaneous game without R's knowledge.

Appendix B

Proof of Proposition 2. I first look at the expected outcome when C chooses column 4 and $V = 1$. The expected outcome, when R chooses R_1 with probability r_1 and R_2 with probability r_2, is r_1 times the element in row 1, column 4 plus r_2 times the element in row 2, column 4; that is,

$$\begin{aligned} &r_1\{PX_{11} + (1 - P)[WX_{12} \\ &\quad + (1 - W)X_{11}]\} + r_2\{Q[WX_{22} \\ &\quad + (1 - W)X_{21}] + (1 - Q)X_{21}\} \\ &= r_1PX_{11} + r_1WX_{12} - r_1PWX_{12} \end{aligned}$$

Figure A-3. Simultaneous-Sequential Game

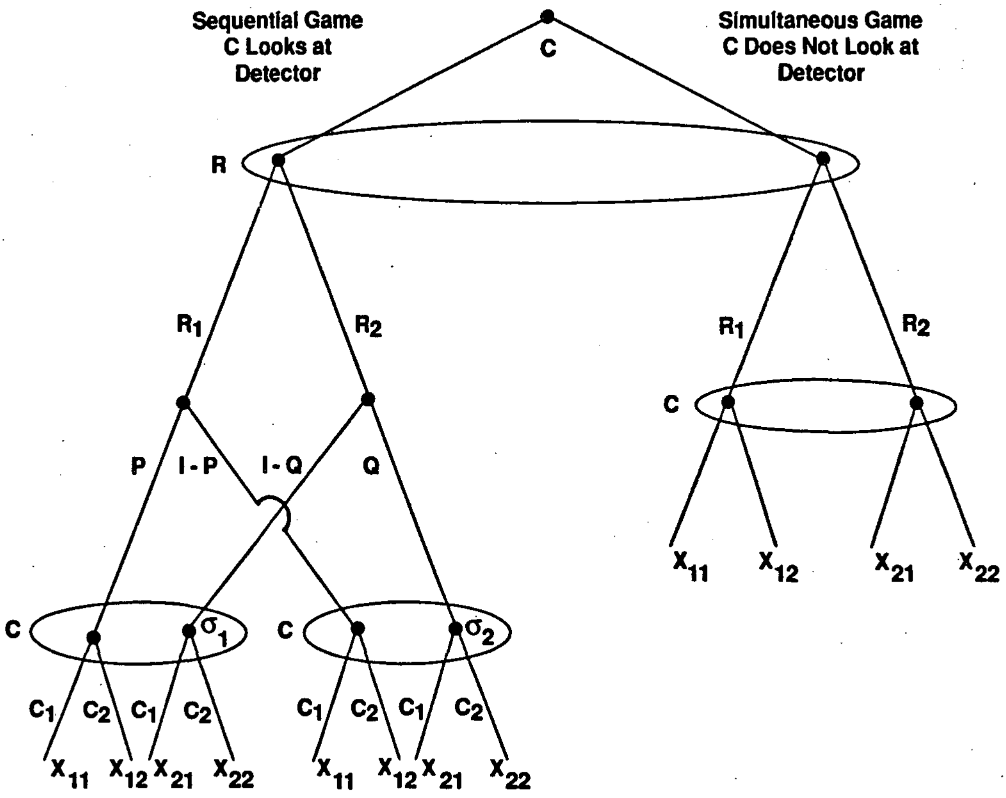

Sequential Game
C Looks at
Detector
C
Simultaneous Game
C Does Not Look at
Detector
R
R_1
R_2
R_1
R_2
C
P
I - P
I - Q
Q
X_{11} X_{12} X_{21} X_{22}
C
σ_1
C
σ_2
C_1 C_2 C_1 C_2
C_1 C_2 C_1 C_2
X_{11} X_{12} X_{21} X_{22}
X_{11} X_{12} X_{21} X_{22}

Table A-1. The Symmetric Game

	C Honest			
	C Does Not Look at Detector			
Arms Control Agreement	*C* Assumes *R* Honest (C_1, C_1)	*C* Assumes *R* Cheated (C_2, C_2)	*C* Uses *P*% Detector (C_1, C_2)	*C* Cheats
R honest				
R assumes *C* honest (R_1, R_1)	X_{11}, Y_{11}	X_{33}, Y_{33}	$PX_{11} + (1 - P)X_{33}$, $PY_{11} + (1 - P)Y_{33}$	X_{12}, Y_{12}
R assumes *C* cheated (R_2, R_2)	X_{33}, Y_{33}	X_{33}, Y_{33}	X_{33}, Y_{33}	X_{33}, Y_{33}
R uses *P*% detector (R_1, R_2)	$PX_{11} + (1 - P)X_{33}$, $PY_{11} + (1 - P)Y_{33}$	X_{33}, Y_{33}	$P^2X_{11} + (1 - P^2)X_{33}$, $P^2Y_{11} + (1 - P^2)Y_{33}$	$PX_{33} + (1 - P)X_{12}$, $PY_{33} + (1 - P)Y_{12}$
R cheats	X_{21}, Y_{21}	X_{33}, Y_{33}	$PX_{33} + (1 - P)X_{21}$, $PY_{33} + (1 - P)Y_{21}$	X_{33}, Y_{33}

Note: $X_{21} > X_{11} > X_{33} > X_{12}$; $Y_{12} > Y_{11} > Y_{33} > Y_{21}$.

$$\begin{aligned} &+ r_1X_{11} - r_1WX_{11} - r_1PX_{11} \\ &+ r_1PWX_{11} + r_2QWX_{22} + r_2QX_{21} \\ &- r_2QWX_{21} + r_2X_{21} \\ &- r_2QX_{21}. \qquad \textbf{(A-1)} \end{aligned}$$

I will compare this outcome to the expected outcome when *C* chooses column 3 with probability W' and column 1 with probability $1 - W'$, and *R* choose R_1 with probability r'_1 *and* R_2 with probability r'_2:

$$\begin{aligned} &r'_1(1 - W')X_{11} + r'_1W'[PX_{11} \\ &\quad + (1 - P)X_{12}] + r'_2(1 - W')X_{21} \\ &\quad + r'_2W'[QX_{22} + (1 - Q)X_{21}] \\ &= r'_1X_{11} - r'_1W'X_{11} + r'_1W'PX_{11} \\ &\quad + r'_1W'X_{12} - r'_1W'PX_{12} + r'_2X_{21} \\ &\quad - r'_2W'X_{21} + r'_2W'QX_{22} \\ &\quad + r'_2W'X_{21} - r'_2W'QX_{21}. \qquad \textbf{(A-2)} \end{aligned}$$

Setting $r'_i = r_i$ and $W = W'$, it is readily established that equations A-1 and A-2 are equivalent. A similar process demonstrates that a mixed strategy between columns 2 and 3 is equivalent to choosing column 4 with $W = 1$ and $0 < V < 1$. These mixed strategies are superior to a mixed strategy between columns 1 and 2. I will provide the intuition, which should make the result obvious. Assume that *C* chooses column 1 with probability *P* and column 2 with probability $1 - P$. If *R* chooses r_1, *C*'s expected payout is the same as it would be if *C* had chosen column 3 with probability 1. However, if *R* chooses r_2, *C*'s expected payment is $PX_{21} + (1 - P)X_{22}$. This is greater than $QX_{22} + (1 - Q)X_{21}$, *C*'s expected payment when *C* chooses column 3 with certainty (and *R* has chosen r_2), since $P, Q > 1/2$ and $X_{21} > X_{22}$. Since *C* wants to minimize payments, column 3 dominates this particular mixed strategy between columns 1 and 2. Hence, any other mixed strategy between columns 1 and 2 is dominated by another mixed strategy between columns 3 and 1 or 2.[31] The rationale for *R*'s taking a mixed strategy is most apparent when *C* chooses a mixed strategy between columns 1 (or 2) and 3. No one row dominates. Therefore *R* chooses a mixed strategy. QED

Appendix C: The Symmetric Game of Detection with Imperfect Detection

I allow for imperfect monitoring of *C* by *R*, thereby creating a symmetric relationship between the two players. Looking at Table A-1, I make the following additional notational conventions and assumptions:

1. If either side is honest with regard to arms control but believes, either by detection (possibly mistaken) or by assumption, that the other side has cheated before it is too late to do anything about it, the arms control agreement is broken and the outcome reverts to that of no arms control (X_{33}, Y_{33}). Note that I assume that the outcome of a failed agreement will be X_{33}, Y_{33}; that is, I do not distinguish between the minor variations in X_{33}, Y_{33} as I did in the previous section. Ignoring these minor variations allows us to concentrate on the more complex issues (however, the reader can readily extend the framework of the analysis to include these variations).

2. If one side has successfully avoided detection of its violation until it is too late for the other side to do anything about it, the violator will present the other side with the facts of its violation and make demands on the other side based on its superior position.[32] (a) If the other side has also cheated, it will provide information that it too has cheated, and the outcome reverts to no arms control (X_{33}, Y_{33}); (b) If the other side has not cheated, the outcome reflects the relative advantage of the one side over the other (X_{21}, Y_{21} if *R* has cheated and not *C*; and X_{12}, Y_{12} if *C* has cheated and not *R*). Note that a and b mean that there is only one strategy choice by a player if it has cheated, as opposed to three strategy choices if the player has not cheated.

3. $X_{12} < X_{ij}$ and $Y_{12} > Y_{ij}$ for all $ij \neq 12$. This is the symmetric relationship to X_{21}, Y_{21}.

From Table A-1, it is apparent that column 4 dominates column 2 and row 4 dominates row 2; that is, an optimal strategy will never involve assuming that the other side has cheated (without looking at the detection equipment). Thus the matrix can be simplified as in Table A-2. For $P < 1$, once again one can expect that randomized strategies will be undertaken with there being some positive probability of each side's cheating and some positive probability of each side's ignoring its detection equipment.

Appendix D: The Effect of Monitoring Quality on *C* and *R*'s Behavior

Letting c_1 stand for the probability of *C*'s choosing column 1 and r_1 stand for the probability of *R*'s choosing row 1, *C*'s expected utility is

$$\begin{aligned} W^C = {} & c_1 r_1 Y_{11} + (1 - c_1) r_1 [P Y_{11} \\ & + (1 - P) Y_{\bar{3}\bar{3}}] + c_1 (1 - r_1) Y_{21} \\ & + (1 - c_1)(1 - r_1)[P Y_{\bar{3}\bar{3}} \\ & + (1 - P) Y_{21}]. \qquad \text{(A-3)} \end{aligned}$$

First-order interior conditions for *C* are[33]

$$\begin{aligned} W^C_{c_1} = {} & r_1 Y_{11} - r_1 [P Y_{11} + (1 - P) Y_{\bar{3}\bar{3}}] \\ & + (1 - r_1) Y_{21} - (1 - r_1)[P Y_{\bar{3}\bar{3}} \\ & + (1 - P) Y_{21}] \\ = {} & r_1 (1 - P)(Y_{11} - Y_{\bar{3}\bar{3}}) \\ & + (1 - r_1) P (Y_{21} - Y_{\bar{3}\bar{3}}) = 0. \qquad \text{(A-4)} \end{aligned}$$

I will make use of these first-order conditions (equation A-4) in establishing the

Table A-2. The Symmetric Game in a Concise Form

Arms Control Agreement	*C* Honest		*C* Cheats
	C Assumes *R* Honest	*C* Uses *P*% Detector	
R honest			
R assumes *C* honest	X_{11}, Y_{11}	$PX_{11} + (1 - P)X_{33},$ $PY_{11} + (1 - P)Y_{33}$	X_{12}, Y_{12}
R uses *P*% detector	$PX_{11} + (1 - P)X_{33},$ $PY_{11} + (1 - P)Y_{33}$	$P^2X_{11} + (1 - P^2)X_{33},$ $P^2Y_{11} + (1 - P^2)Y_{33}$	$PX_{33} + (1 - P)X_{12},$ $PY_{33} + (1 - P)Y_{12}$
R cheats	X_{21}, Y_{21}	$PX_{33} + (1 - P)X_{21},$ $PY_{33} + (1 - P)Y_{21}$	X_{33}, Y_{33}

Note: $X_{21} > X_{11} > X_{33} > X_{12}; Y_{12} > Y_{11} > Y_{33} > Y_{21}$.

following equivalence and definitional relationships:

$$\frac{r_1(1 - P)}{P(1 - r_1)} = \frac{Y_{\bar{3}\bar{3}} - Y_{21}}{Y_{11} - Y_{\bar{3}\bar{3}}} \equiv K. \quad \text{(A-5)}$$

We now turn our attention to the relationship of r_1 to K and P.

Proof of Proposition 4, Part 1. From equation A-5 we know that $[r_1(1 - P)]/[(1 - r_1)P] = K$. This relationship can be solved for r_1 by the following steps:

$$r_1 = \frac{PK - r_1PK}{1 - P} = \frac{PK}{1 - P} - \frac{r_1PK}{1 - P}.$$

Equivalently,

$$r_1\left(1 + \frac{PK}{1 - P}\right) = r_1\left(\frac{1 - P + PK}{1 - P}\right)$$
$$= \frac{PK}{1 - P},$$

or

$$r_1 = \frac{PK}{1 - P + PK}$$
$$= \frac{P}{[(1 - P)/K] + P}. \quad \text{(A-6)}$$

From the last equality it is clear that (1) as K increases, so does r_1, (2) as K decreases toward 0, r_1 approaches 0, (3) when $K = 1$, $r_1 = P$, and (4) as K increases toward infinity, r_1 approaches 1. QED

Appendix E

Proof of Proposition 5, Part 1. R's expected utility is

$$\begin{aligned} W^R = {} & c_1r_1X_{11} + (1 - c_1)r_1[PX_{11} \\ & + (1 - P)X_{\bar{3}\bar{3}}] + c_1(1 - r_1)X_{21} \\ & + (1 - c_1)(1 - r_1)[PX_{\bar{3}3} \\ & + (1 - P)X_{21}]. \end{aligned}$$

First-order interior conditions for R are:

$$\begin{aligned} W^R_{r_1} = {} & c_1X_{11} + (1 - c_1)[PX_{11} \\ & + (1 - P)X_{\bar{3}\bar{3}}] - c_1X_{21} \\ & - (1 - c_1)[PX_{\bar{3}3} + (1 - P)X_{21}] \\ & = 0. \end{aligned} \quad \text{(A-7)}$$

In order to find the effect of P on c_1 I take the total differential of equation A-7.

$$\begin{aligned} & [X_{11} - PX_{11} - (1 - P)X_{\bar{3}\bar{3}} - X_{21} \\ & + PX_{\bar{3}3} + (1 - P)X_{21}]dc_1 \\ & + (1 - c_1)(X_{11} - X_{\bar{3}\bar{3}} - X_{\bar{3}3} \\ & + X_{21})dP = 0. \end{aligned} \quad \text{(A-8)}$$

Rearranging equation A-8, I get the following relationship:

$$[(1 - P)(X_{11} - X_{\bar{3}\bar{3}}) + P(X_{\bar{3}3} - X_{21})]dc_1 = -(1 - c_1)(X_{11} - X_{\bar{3}\bar{3}} - X_{\bar{3}3} + X_{21})dP. \qquad \textbf{(A-9)}$$

On the right-hand side of the equality, it is clear that the sign of the expression is negative, since X_{11}, $X_{21} > X_{\bar{3}3}$, $X_{\bar{3}\bar{3}}$ by assumption. I will show that the left-hand side of the equality is also negative so that an increase in P results in an increase in c_1. Earlier (in the conditional statement of proposition 3, part 2) I assumed that

$$P(X_{11}) + (1 - P)X_{\bar{3}\bar{3}} > P(X_{\bar{3}3}) + (1 - P)X_{21}. \qquad \textbf{(A-10)}$$

Rearranging equation A-10 I obtain the following relationships:

$$0 > -PX_{11} + PX_{\bar{3}3} + (1 - P)(X_{21} - X_{\bar{3}\bar{3}}) = (1 - P)(X_{11} - X_{\bar{3}\bar{3}}) - X_{11} + P(X_{\bar{3}3} - X_{21}) + X_{21}.$$

$$> (1 - P)(X_{11} - X_{\bar{3}\bar{3}}) + P(X_{\bar{3}3} - X_{21})$$

$$= \text{the bracketed term on the left-hand side of equation A-9.}$$

The last inequality holds because $X_{21} > X_{11}$ by assumption. Thus, the left-hand side of equation A-9 < 0, and P and c_1 move in the same direction.

Proof of Part 2. In order to find the effect of a change in both $X_{\bar{3}3}$ and $X_{\bar{3}\bar{3}}$ on c_1 I take the total differential of equation A-7, treating the change in $X_{\bar{3}3}$ as equivalent to the change in $X_{\bar{3}\bar{3}}$.

$$[(1 - P)(X_{11} - X_{\bar{3}\bar{3}}) + P(X_{\bar{3}3} - X_{21})]dc_1 = -(1 - c_1)(1 - 2P)dX_{\bar{3}3}.$$

Once again the left-hand side is negative. The right-hand side is positive since $P >$ 1/2 by assumption. Therefore as $X_{\bar{3}3}$ and $X_{\bar{3}\bar{3}}$ increase (decrease), c_1 decreases (increases). QED

Proof of Part 3. This follows the same logic and is left to the reader.

Appendix F

Proof of Proposition 6. From proposition 3, if an arms control equilibrium exists, it must involve mixed strategies by C (between columns 1 and 3) and by R (between rows 1 and 2). At equilibrium, R is indifferent between rows 1 and 2. From row 1 in Table 2, R strictly prefers X_{11} to $PX_{11} + (1 - P)X_{\bar{3}\bar{3}}$. Therefore, the greater the probability of C's choosing column 3 in equilibrium, the lower R's expected utility. If the probability of C's choosing column 3 were 1 (or very close to it), R's expected utility would only be $PX_{11} + (1 - P)X_{\bar{3}\bar{3}}$. But even under these circumstances, R's expected utility would be greater than X_{33} by assumption. QED

Appendix G

Proof of Proposition 7. At equilibrium, C is indifferent between columns 1 and 3. This indifference is an immediate consequence of rearranging the first-order conditions for C (equation A-3) as follows:

$$r_1Y_{11} + (1 - r_1)Y_{21} = r_1[PY_{11} + (1 - P)Y_{\bar{3}\bar{3}}] + (1 - r_1)[PY_{\bar{3}\bar{3}} + (1 - P)Y_{21}]. \qquad \textbf{(A-11)}$$

In order to calculate C's expected utility, I substitute $PK/(1 - P + PK)$ for r_1 (from equation A-6) into the right-hand side of equation A-1.

$$W^C = [PK/(1 - P + PK)][PY_{11} + (1 - P)Y_{\bar{3}\bar{3}}] + [(1 - P)/(1 - P + PK)][PY_{\bar{3}\bar{3}} + (1 - P)Y_{21}]$$

$$= [P^2KY_{11} + PKY_{\bar{3}\bar{3}} - P^2KY_{\bar{3}\bar{3}} + PY_{\bar{3}\bar{3}} - P^2Y_{\bar{3}\bar{3}} + (1-P)^2Y_{21}]/(1-P+PK). \quad \textbf{(A-12)}$$

$$K \equiv \frac{Y_{\bar{3}\bar{3}} - Y_{21}}{Y_{11} - Y_{\bar{3}\bar{3}}}.$$

Substituting this expression into the first and third terms in equation A-12, I get

$$[P^2Y_{\bar{3}\bar{3}} - P^2Y_{21} + Y_{\bar{3}\bar{3}}(KP + P - P^2) + (1 + P^2 - 2P)Y_{21}]/(1 - P + PK) = [Y_{\bar{3}\bar{3}}(KP + P) + (1-2P)Y_{21}]/(1-P+PK). \quad \textbf{(A-13)}$$

For an arms control equilibrium to be preferred by C to the equilibrium under no arms control, the right-hand side of equation A-13 must be greater than Y_{33}. QED

Notes

1. See Hirshleifer 1987 for an interesting comparison of sequential move games to simultaneous move games. He does not incorporate both games into one general game, as I do here.

2. While the words *signal* and *detection* have been used in statistics, the analysis here is quite different, since I analyze game playing by both participants.

3. Many theoretical results depend upon the relative size of the various parameters, the relative size being determined by empirical studies.

4. If one row (or column) dominated another row (column), there would be no need for detection equipment, as the other side's optimal choice would always be known.

5. These signals are not under any control by R; rather, any action by R has some potential of being observed.

6. The differing error rates, $1 - P$ and $1 - Q$, correspond to type 1 and type 2 errors in statistics. See Richelson 1979 for a discussion of detection vs. false alarms.

7. At the top of column 1, vector C_1, C_1 indicates that C chooses C_1 whether σ_1 or σ_2 has occurred.

8. However, as will be shown, the best response to σ_i is not choosing C_i with certainty.

9. The effect of exogenous changes on the strategies by R and C, such as the quality of the monitoring equipment and the size of X_{ii}, can be derived. For example, it can be shown that a reduction in the probability of a type 1 error has the same qualitative change on the strategies of the players as a reduction in the probability of a type 2 error (see Wittman 1987).

10. This trade-off curve is closely related to the receiver operating characteristic curve (used by O'Neill [1988]) which treats the probability of X_{12} as a function of the probability of X_{22}. One can also view row's strategy as being deterministic rather than random. For example, row 1 might be sending 10 planes north, while row 2 might be sending 10 planes south. A mixed strategy might be to choose row 1 with probability .6 and row 2 with probability .4. The deterministic equivalent is to have 6 planes flying north and 4 planes flying south (assuming that there are constant returns to scale).

11. In the post-World War II time period, the Soviet Union did not publish truthful population statistics because it feared that other countries would realize how weak it was in military manpower. Some believe that the Soviets were against on-sight inspection in the past because it would have revealed that they were militarily very weak.

12. The phrase "C believes that R has cheated" stands for the phrase that C undertakes strategy C_2, which I assume is the best strategy given that R cheats (R_2). C_2 may be the strategy choice arising from either the simultaneous game or the sequential game, thus "C believes" means C acts as if C believed that R has cheated.

13. X_{11} and Y_{11} are functions of the economic costs and political power benefits from armaments. For a discussion of these factors and the appropriate functional forms see Wittman 1987. An agreement to eliminate nuclear weapons may actually make both sides worse off if the probability of war increases substantially when the threat of a nuclear second strike is absent. I will not discuss this possibility in greater detail, since my attention is to cases in which arms reduction is desired by both sides if fully enforceable and perhaps desired even if not fully enforceable.

14. While this is the perceived problem, arguments could be made to the contrary if the extra military advantage from cheating on an arms control agreement were outweighed by the extra cost of the armaments.

15. I assume that the detection, if it takes place, occurs before R can gain any advantage by cheating. A more general model might make the probability of detection and the cost to C from R's cheating a function of time. I will not consider these extensions here, in order to prevent the analysis from getting out of control by considering too many alternatives.

16. Once again, I do not distinguish how this "belief" arose. If C chooses C_i, the outcome is the same whether or not C looked at the detection equipment before making the decision.

17. If R were not intending to make use of its arms, R's cheating would not hurt C. However, I have assumed that C is honest. Therefore, there

would be no gain to *R* in cheating unless *R* were intending to use its arms against C. Some authors have argued that the issue of verification (protection against cheating) is just a smoke screen. However, for certain kinds of potential limitations and certain types of weapon systems, the ability to monitor at all accurately is extremely problematical. For example, a cruise missile (both in the testing stage and in deployment) is harder to detect than an ICBM silo; research on "Star Wars" weapons is harder to monitor than is their testing; and monitoring the production of nuclear warheads is much more difficult than detecting nuclear explosions in space. Indeed, Earle (1986) argued that 85%–95% of the verbiage in SALT II was devoted to the issue of verification. Furthermore, as I shall show, an arms control agreement may not achieve an equilibrium different from the outcome encompassing no agreement unless there is a difference in verification.

18. It is possible that for propaganda purposes *C* may engage in an arms control agreement even though it intends to claim that *R* is cheating soon after the agreement is made. This line of argument suggests that assumption 3 is incorrect and that the reverse is true: $Y_{33} < Y_{33}$. The intention also suggests that $Y_{33} > Y_{11}$. Since I am investigating situations in which an honestly held arms control agreement is preferred to a situation of no arms control, I will not consider this possibility further.

19. The approach used here is considerably different from previous work on verification and arms control. Brams and Davis (1987) and Brams and Kilgour (1986) assume that *R*'s primary goal is to hide the truth and *C*'s primary goal is to discover the truth. I posit different goals for *R* and *C*. Under my formulation, if *R* has been honest with regard to arms control, it does not want to hide the truth. These authors do not consider the possibility that detection is a sequential game with the detector making its move after observing (possibly with error) *R*'s choice. Brams and Kilgour (1988, chap. 8) view detection as a type of deterrence and the possibility of using verification as a threat, inducing a pure strategy response. Fichtner (1986) has a model where the detector works perfectly but is costly to use. Maschler's 1963 model of a test ban agreement does not consider the choice between using the detector and not using the detector, and he specifically avoids probabilistic choice by the participants. Thus, his model does not deal with columns 1, 2, or 4 of Table 2 but is similar to column 3 (although the probabilities and payoffs I use are only implicit in his model; thus, he assumes that the United States will not cheat because it does not want to take the *risk* of being caught). Furthermore he makes different assumptions. For example, to translate into my model, he assumes that column prefers an arms control agreement with cheating by row (but not by column) to the outcome in which there is no arms control agreement. Most of the other literature on inspection is cast as a game in which one side (with a fixed number of inspections available to it) maximizes the probability of catching the other side cheating and the other side minimizes the probability of being caught, given that it is going to cheat. Work by Kuhn (1963) and Davis and Kuhn (1963) fall into this genre. Thus, the objective functions and the game itself are considerably different from that presented here. Harsanyi (1967) applies the power index to arms control and no arms control, respectively, and is not interested in verification. McGuire (1965) shows that in an uncertain world, increased armaments are a substitute for increased knowledge about the other side. Under arms control, information about the other side's arms is provided, thereby allowing for reduced armaments. My work is thus only tangentially related to his.

20. I assume that $X_{\bar{3}3} > X_{33}$. With the inequalities reversed, there would be no pure strategy equilibrium.

21. See Wittman 1987 for the proof of the sufficiency conditions. Note that a mixed strategy equilibrium never involves the choice of having no arms control. No arms control is a different game. Both sides know whether they are in the arms control game (with possible cheating).

22. Improved verification capabilities (higher *P*) may have been the reason why SALT I was implemented in 1972 rather than earlier.

23. This is restating Proposition 4, part 1 in a slightly different way. Since $r_1 = P$ when $K = 1$ and increases as *K* increases, $r_1 < P$ for $K < 1$. Equivalently, $1 - r_1 > 1 - P$ for $K < 1$.

24. Note that this is a necessary but not sufficient condition for an arms control agreement. Bargaining over the terms of the agreement may take a very long time and in some cases may prevent an agreement from being reached. See Wittman (1979) for a discussion of necessary conditions for an agreement to end a war.

25. Aspin and Kaplan (1980) made similar statements regarding SALT: " 'Adequate verification' means verification sufficient to assure that the uncertainties in verifying an arms control treaty creates fewer risks than those facing the United States in a world without the treaty."

26. Of course, when detection is difficult and the cost of not detecting violations is high, an agreement is less likely.

27. If simultaneous behavior is not necessary, technological changes in monitoring capabilities may result in partial unilateral disarmament. An analysis of the choice between unilateral and bilateral agreements is a topic that deserves more attention.

28. The hostages need not be people. See Williamson 1983.

29. Stiglitz and Weiss (1983) considered the possibility of (job) terminations, the analog to the dissolution of an arms control agreement. However,

they did not consider the possibility of any error in detection.

30. In fact, I have shown that an optimal strategy increases certain types of risk.

31. Since R has only two pure strategies, an optimal strategy by C would not involve a mixed strategy between column 4 (which is, itself, a mixed strategy) and another column.

32. See Katz (1980) for a depiction of such a scenario after the USSR has violated the SALT agreement. See also Tsipis, Hafemeister, and Janeway 1986.

33. The pure strategy solutions are corner solutions.

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RESEARCH NOTE

DIMENSIONS IN CONGRESSIONAL VOTING

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While dimensional studies of congressional voting find a single, ideological dimension, regression estimates find several constituency and party dimensions in addition to ideology. I rescale several unidimensional studies to show their increased classification success over the null hypothesis that votes are not unidimensional. Several null hypotheses are explored. With these null hypotheses, 66%–75% of nonunidimensional roll call votes are nevertheless correctly classified by one dimension. After the rescaling, one dimension succeeds in correctly classifying 25%–50% of the votes, and second and third dimensions are important.

Recent dimensional studies of aggregate congressional roll call voting have found a single, left-right, ideological dimension (Poole 1981; Poole and Rosenthal 1985a, 1985b; Schneider 1979, 1987).[1] However, regression estimates of roll call votes have found numerous constituency and party dimensions (Kau, Keenan, and Rubin 1982; Peltzman 1984, 1985), with ideology at best a secondary factor (e.g., Kalt and Zupan 1984, n.d.). These results are contradictory: a single dimension either does or does not explain roll call votes. I examine the dimensional methodology and find that the power of a single dimension has been overstated.

I first describe dimensional findings prior to the recent unidimensional studies, then show how the assumptions of dimensional analysis overstate unidimensionality, largely by using a success criterion that overstates both the overall success of the dimensional method and the first dimension's relative fit to later dimensions. A null hypothesis is developed; it shows the classification success of unidimensionality when the data are *not* unidimensional. Adjusted success ratios then show the *improvement* in classification above this level. They are calculated for several dimensional studies. I conclude by showing how a method that finds a single dimension might be useful for different purposes than a method finding many dimensions.

Dimensional studies require some form of abstract space. In dimensional analysis the space is defined by the data, while in regression analysis a space is defined a priori, usually with many dimensions. Regression analysis of voting has followed the economic paradigm of demand. As demand "can be expressed as a function of all prices and income" (Russell and Wilkinson 1979, 52), in principle there are as many dimensions as goods in the economy. So regression estimates of roll call voting include the varied demands of different constituents and pressure groups and find many dimensions.

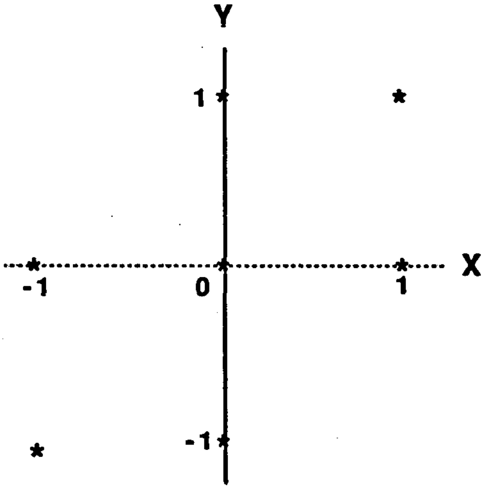
If measurable basic dimensions are not known a priori, dimensional techniques can organize data such as roll call votes into dimensions according to a statistical criterion. The data and the organizing principle determine the dimensions. Such

Table 1. Classification Success of Dimensional Methods: An Example

Case	X	Y	A	B	C
1	-1	-1	1	0	0
2	-1	0	1	1	0
3	0	-1	0	-1	0
4	0	0	0	0	0
5	0	1	0	1	0
6	1	0	0	-1	1
7	1	1	0	0	1

Note: $X = -A + C$; $Y = -A + B + C$.

Figure 1. One Dimension Fit to Two-Dimensional Votes



techniques include factor analysis, multidimensional scaling, and Coombs' (1964) and Poole and Rosenthal's (1985a, 1985b; Poole 1984, 1988) *unfolding*.

These methods severely limit the number of dimensions. First, dimensions are usually found by a stepwise method. One dimension is estimated, and its explanatory power calculated. A second dimension is estimated to fit the remaining errors, and its improvement on the first variable is calculated. As with stepwise regression, earlier dimensions are correlated. Jointly explained variation is assigned to earlier dimensions. Second, the dimensions are sometimes required to be orthogonal, although actual dimensions are invariably correlated. This forces later dimensions away from their best fits to the data.

Dimensional analysis often finds dimensions that seem to represent political and economic interests, ideology, and so on; the dimensions represent causal factors. They can be identified by their correlations with variables like Americans for Democratic Action (ADA) or political action committee ratings (Poole and Daniels 1985).

A numerical example illustrates some problems with dimensional fitting. The seven observations are shown in Table 1 and Figure 1. They could be legislators' bliss points in a two-dimensional issue space or *yes* (1) and *no* (-1) votes and abstentions (0) on two bills. Consider them true values, which the statistical methods try to describe (Coombs' [1964] relations between pairs of dyads, category Ia).

How can the points be fit to a single dimension? The single dimension that classifies best is $X = Y$ with the cleavage $Y = -X$. It correctly categorizes 12 of the 14 votes, counting abstentions that are off the cleavage as half correct. Using regression as a basis of comparison, however, the R-squared for the regression $X = Y$ is zero (sum of squared residuals [SSR] = 4, while SSR for $Y = 0$ is also 4). The regression with Y the dependent variable is $Y = .5X$, with $R^2 = .25$ (SSR = 3), but it correctly categorizes only 10 of the 14 votes.

Other independent variables—say A, B, and C—could describe the legislators' choices. The Y votes are perfectly estimated by the line $Y = -A + B + C$, implying three dimensions. But regression can overstate the number of required dimensions: since $X = -A + C$, the two-dimensional regression $Y = X + B$ gives a perfect fit. Yet dimensional analysis also fails to fit a second dimension properly if it is fitted to the residuals from the first dimension (the points off $X = Y$); two lines ($X = -Y$, $X = 0$) do equally well, but each leaves one point incorrectly classified.

Thus, dimensional methods can seem more successful than they really are (as with $R^2 = 0l$), and they distort additional dimensions.

Multidimensional Findings

The finding of recent dimensional studies that a single dimension explains congressional roll calls is puzzling. Do citizens and politicians care about only one great issue? Earlier work that found several dimensions in congressional voting can put these results in perspective.

The major multidimensional findings are by Clausen (Clausen and Cheney 1970; Clausen 1973); Collie (1985) provides a review. Issue dimensions are identified a priori; their existence is confirmed by finding that votes are highly correlated within each dimension but have low correlations across dimensions. However, no one has tested the relative statistical success of one and several dimensions with this method by placing these dimensions in a higher-dimension space and calculating whether a unidimensional reduction of the space would have as good statistical properties as the a priori multiple dimensions. If the policy dimensions do capture underlying differences, a single dimension should fit poorly. If the correlations across dimensions are high, as Jerrold Schneider (1979, 134) found between economic issues and foreign affairs and as William Schneider reported across economic and social issues and foreign affairs,[2] one dimension might categorize votes nearly as well as several dimensions.

However, multiple dimensions can also have theoretical value, as Figure 2 (from Sundquist 1983) shows. Sundquist (1983) uses two dimensions to show how new, cross-cutting cleavages occur in U.S. politics. He starts with a one-dimensional cleavage, but then a second cross-cutting issue develops (slavery), causing voting on both dimensions. To show why a specific group of voters realigned, Sundquist needs a new issue dimension. But after the new cleavage becomes stable, voting is on the apparently one-dimensional cleavage *B–C*.

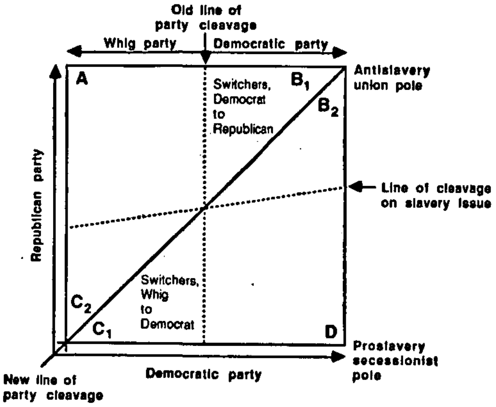
Thus, after the New Deal, southern Democrats faced cross-cutting cleavages. They were allied with northern Democrats on New Deal spending (Peltzman 1985), while the one party system, civil rights, and foreign policy divided them from the North—dimensions separate from economic ideology (Collie and Brady 1985). Explaining southern roll call votes should require both economic and other variables: even if a one-dimensional liberal-conservative scale fits most votes, it misses the underlying forces that caused the votes.

A Null Hypothesis for Unidimensional Unfolding

Unidimensional unfolding seems to classify votes very successfully (Ladha 1984; Poole 1984; Poole and Daniels 1985; Poole and Rosenthal 1985a, 1985b). But "success" depends upon an appropriate null hypothesis to show the "success" of the model with data contrary to the model's assumptions (Weisberg 1978). We now develop such null hypotheses and calculate their success in classifying votes. Correct categorization, rather than minimizing error (distance), is the success criterion used here, following most of the dimensional voting studies. It is reasonable for inherently 0–1 variables.

Figure 2 can illustrate this approach with two equally important issue dimensions, assuming that voters are distributed equally across all four quadrants, so voters' positions on the two issues are uncorrelated. Suppose that there are equal numbers of bills on each issue and voters

Figure 2. Unidimensional Voting in a Two-Dimensional Space (Realignment of the 1850s)


Old line of party cleavage
Whig party
Democratic party
A
Switchers, Democrat to Republican
B_1
B_2
Antislavery union pole
Line of cleavage on slavery issue
Republican party
Switchers, Whig to Democrat
C_2
C_1
D
Proslavery secessionist pole
New line of party cleavage
Democratic party

Source: Sundquist 1983, Fig. 5-2.

choose the issue position closer to their own.

While these issues are uncorrelated, if "left" is defined as corner *A* and "right" is corner *D*, 75% of the votes are correctly classified by the *B–C* cleavage that divides voters into left and right. So one dimension gives 75% "success" even though no one votes according to ideology (the *A–D* dimension). Thus, a single dimension could correctly classify 75% of votes when there are actually two dimensions. Also, while adding a second dimension gives 100% success, the first dimension would be credited with three-quarters of the predictive power, and neither dimension is a true issue dimension.

A null hypothesis has "random" behavior with respect to the model's assumptions. It gives a baseline success level, the classification success of the dimensional model when the data do not fit the model. We can then know how much more successfully the dimensional model classifies the actual voting data. The natural null hypothesis for a single dimension is a perfectly symmetrical distribution of votes in two dimensions. It assumes *zero correlation* between legislators' positions on the issues. (As zero correlation is improbable, this assumption is biased *in favor of* the dimensional estimates: correlation gives higher success for the null hypothesis. Another possible null hypothesis is that all correlations are equally probable. Since correlation ranges between 1 and −1 and in voting models all correlations are counted as positive, the average correlation would be .5.)[3]

Figure 3 shows the null hypothesis with two issue dimensions, orthogonal preferences, and 16 legislators. Their "bliss" points, shown as small circles, are distributed evenly around a circle in the two-dimensional issue space. Legislators vote for the position closer to their bliss point. Clearly, the correlation of legislators' positions on the two issues is zero. One bill is now introduced at each legislator's bliss point; each divides the legislators into two equal groups (as in a minimum winning coalition). Thus, each cleavage line passes through the center, and the cleavages are distributed evenly around the circle, as shown in the figure. With these assumptions, there should be no evidence for a single left-right dimension.

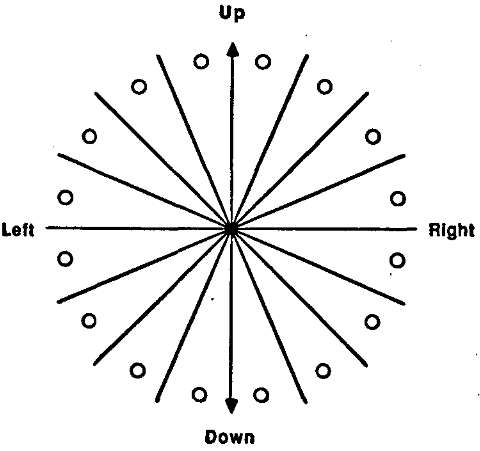
Yet the prediction that bills divide left from right has 75% success. Arbitrarily call the left half of the legislators in Figure 3 "leftists" and the right half "rightists." (Any other division does just as well.) The bill dividing legislators on the vertical cleavage gives a 100%-correct left-right classification: all of the leftists are on one side and all of the rightists are on the other. The bill on the next cleavage has 87.5% correct classification (14 of 16 legislators). The bills on the succeeding cleavages have 75%, 62.5%, 50%—and then 62.5%, 75%, and 87.5%—success. Since the left-right direction is inferred from the data and maximizes correct classification, 50% is the lowest possible success ratio. Average classification success is 75%, not far below the unidimensional success rates of 81%–87% in Poole and Rosenthal 1985b and Poole and Daniels 1985 (see Tables 2, 3, and 4).

If the legislators' views on the two issues are not orthogonal but rather have the "average" correlation of 50%, a still-higher success rate is obtained. Figure 4 shows eight legislators' bliss points (*) in a two-dimensional space. Only two bills need be considered, one left-right and one up-down. Correlated tastes need not change the success of the left-right cleavage, which is still 75% (100% on the left-right bill, 50% on the up-down bill). But the most successful single dimension is A–B, which gives a cleavage, $Y = -X$, that correctly classifies 87.5% of the votes (taking votes on the cleavage as half correct).

If legislators are randomly distributed but bills are not, the predictive success of one dimension will increase and its location will depend on the choice of bills. So dimensional methods identify the distribution of bills as much as the distribution of preferences.[4] For a dimension to show only legislators' preferences, either roll call votes must be distributed in proportion to the relative importance of issues, or all bills must be equally important. If many roll call votes are on a minor issue, the dimension's direction is likely to change.

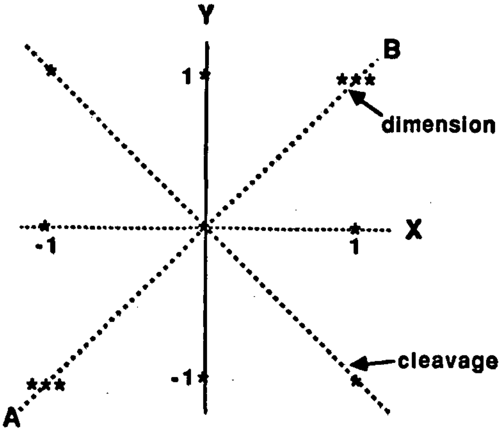
These points are illustrated in Figure 3. If only pure left-right bills are proposed, classification success is 100%. If half are pure left-right and half are distributed equally across all alternatives, success is the average of 100% and 75%—87.5%. Now, Poole and Rosenthal's (1985b) average success for all roll calls is 82.83% (Table 2). If these votes were either perfectly left-right or random in two dimensions, 31% would be on the left-right dimension and 69% random ($\alpha 100 + [1 - \alpha]\ 75 = 82.83$). However, mostly unidimensional votes may not be exactly on that dimension. In Figure 3 they could be equally distributed on the 100% left-right

Figure 3. The Null Hypothesis of Legislators and Cleavages in Two-Dimensions



Note: Small circles represent legislators' most preferred, or "bliss" points. Lines show cleavages dividing legislators into two equal-sized groups.

Figure 4. Classification Success with Correlated Votes



and the two 87.5% cleavages, making their average 93.75% correct. (If legislators make mistakes in voting, the same result holds.) Now, with the same 82.83%-correct classification, 42% are on the left-right cleavage.

Thus, classification success shows the distribution of cleavages on bills but *not* the underlying distribution of preferences. Schneider (1979, 97) emphasizes this point, that Congress has a "truncated agenda" that excludes bills the (liberal) minority prefer. (Bills by *all* small minorities are probably excluded from Congress's agenda, creating a bias toward relatively "centrist" bills or toward those of the dominant coalition.) Actual roll calls are therefore only a small, biased subset of those suggested by the underlying preferences, a result VanDoren (1986) shows in detail for energy bills.

An example can show the importance of a truncated sample. Suppose that in Figure 3 there were "agenda control" by an individual on the left-right axis, who vetoes bills relatively unrelated to the left-right cleavage (Shepsle 1979), leaving only relatively left-right bills. This veto extends to the half of the potential bills in Figure 3 that are most orthogonal to the

Table 2. Unidimensional Findings Recalibrated for 75% and 66.7% Success of Null Hypothesis

	Correct Classification		
		Recalibrated	
Study	Reported (One Dimension)	75% Null Hypothesis (%)	66.7% Null Hypothesis (%)
Poole-Rosenthal 1985b			
All roll calls			
Senate	81.63	26.5	44.8
House	84.03	36.1	52.1
< 60% majority			
Senate	83.86	35.4	51.5
House	82.50	30.0	47.4
Ladha 1984			
1977 Senate roll calls			
Linear model	80.7	22.8	42.0
Nonlinear model	81.7	26.8	45.0
Poole-Rosenthal model	82.3	29.2	46.8
Categories			
Budget general interest	79.79	19.2	39.3
Budget special interest	79.57	18.3	38.6
Regulation			
General interest	85.99	44.0	57.9
Special interest	80.73	22.9	42.1
Domestic social policy	80.02	20.1	40.0
Defense and foreign policy			
Budget	76.23	4.9	28.6
Resolutions	83.54	34.2	50.6
Government organization	80.05	20.2	40.1
Internal organization of Congress	78.71	14.8	36.1
10 strip-mining roll calls (nonlinear model)	78.15	12.6	34.4
Regression estimate (nonlinear model)	77.46	9.8	32.3

Table 3. Dimensional Findings Recalibrated for 75% and 66.7% Success of Null Hypothesis

	Proportion of Roll Call Votes Correctly Classified							
			Recalibrated					
	Reported		75% Null Hypothesis			66.7% Null Hypothesis		
Roll Call Category	1 Dimension	2 Dimensions	1 Dimension	2 Dimensions	Δ1-2[a]	1 Dimension	2 Dimensions	Δ1-2[a]
House								
All votes	86.9	88.4	47.6	53.6	6.0	60.7	65.2	4.5
> 5% minority	83.6	85.2	34.4	40.8	6.4	50.8	55.6	4.8
> 20% minority	80.8	83.3	23.2	33.2	10.0	42.3	49.8	7.5
> 40% minority	80.8	83.3	23.2	33.2	10.0	42.3	49.8	7.5
Senate								
All votes	85.4	87.7	41.6	50.8	9.2	56.2	63.1	6.9
> 5% minority	83.3	85.7	33.2	42.8	9.6	49.8	57.1	7.3
> 20% minority	81.5	84.2	26.0	36.8	10.8	44.4	52.6	8.2
> 40% minority	80.9	83.6	23.6	34.4	10.8	42.6	50.8	8.2

Source: Poole and Daniels 1985, tbls. 6, 6A, sec. 3.

[a] Δ*i*−*j* shows the classification success of the *j*th dimension.

left-right cleavage. Those remaining give 87.5% success.

The null hypothesis has so far used a two-dimensional issue space to evaluate a one-dimensional cleavage. However, the underlying issue space surely has more than two dimensions. (For example, Poole and Daniels [1985] fit two and three dimensions to roll calls without obtaining a perfect fit. See Table 3.) In choosing a null hypothesis with more than two dimensions, two issues must be addressed: (1) What is the appropriate number of dimensions? and (2) What is the appropriate weighting of the dimensions?

Howard Rosenthal has proposed in conversation that an appropriate null hypothesis is an infinite number of dimensions of equal importance, for which correct classification should be 50%. Weisberg (1978) also notes the 50% criterion—as well as the two- and three-party models—as null hypotheses. However, a null hypothesis should be a close and serious alternative to the hypothesis being tested (usually one dimension, sometimes two or three). An infinite number of insignificant dimensions is not a serious alternative, while one or two or three more dimensions *is*. So we find correct classification by one dimension for fully orthogonal voters in three, four, five, six, and seven dimensions. Figure 2 showed that four voters, arrayed in a square, are enough to solve for two dimensions. This principle is extended to the three-dimensional cube

Table 4. R-squared of Regression Results

Roll Call Category	All Votes
Reported	
1 dimension	.812
Δ1-2[a]	.061
2 dimension	.873
Δ2-3[a]	.014
3 dimension	.887
Recalibrated .5 base	
1 dimension	.624
Δ1-2[a]	.122
2 dimension	.746
Δ2-3[a]	.028
3 dimension	.774

Source: Poole and Daniels 1985, tbls. 6, 6A, sec. 3.

[a] Δ*i*–*j* shows the classification success of the *j*th dimension.

Table 5. Procedure of Hypothetical Correct Classifications

Legislator	Vertex	Left-Right	2's Bill	Correct?
1	0 0 0	L	L	+
2	1 0 0	L	L	+
3	0 1 0	L	R	−
4	1 1 0	R	L	+
5	0 0 1	L	R	−
6	1 0 1	R	L	−
7	0 1 1	R	R	+
8	1 1 1	R	R	+

Note: Arbitrarily, legislators 1 and 8 are the extreme left and right, implying the one-dimensional classification shown. A bill is then proposed for each of the eight legislators, and the percentage classified "correctly" (consistent with the L-R classification) is calculated. For example, legislator 2's proposal (or 7's equivalent bill), classifies 50% of the legislators correctly.

and the four-, five-, six-, and seven-dimensional hypercubes: a voter is located at each vertex; a bill dividing the voters in half is proposed at each legislator's bliss point, and the classification success of an arbitrary left-right cleavage is found. (All cleavages do equally well.) The calculations are described in Table 5. (No general formula is evident, and the percent correctly classified does not always fall with additional dimensions.) The results are

Number of Dimensions	*Success Ratio*
2	.75
3	.625
4	.625
5	.648
6	.595
7	.588

The average for 2–4 dimensions is .67, which seems a reasonable "few-dimension" null hypothesis.

Alternatively, a bill could be proposed on each dimension. We index the vertices $i = 1, \ldots n$ from left to right for an n-dimensional hypercube. One bill on each dimension means one bill for each vertex with index number i, $i = 1, \ldots n$. There is one $i = 1$ bill, one on the $i = 2$ dimension, one on the $i = 3$ dimension, and so on. They might be called the "far left" bill, the "left" bill, the "moderate left" bill, the "centrist" bill, and so on. Thus, there are equal numbers of extreme and moderate bills, while the previous case has, for larger ns, mostly moderate and centrist bills, as shown in Table 5.

For each number of dimensions n, there are 2^n legislators—one at each vertex of the n-dimensional hypercube. The legislators are represented by vectors. The three-dimensional case is represented in Table 5.

Because of the hypercube's symmetry, all points $i = 0, \ldots, n$ vertices from the left vertex have the same success ratio. The number of points i vertices from the left is given by the binomial distribution. While correct classification falls from 100% at the left-right extremes to 50% in the center, no general rule seems available. Correct classifications for $n = 2$–7 and the total percentage correct (i.e., the average, weighted by number of points), are shown in Table 6.

The percentage correctly classified are now

Number of Dimensions	*Success Ratio*
2	.75
3	.75
4	.75
5	.75
6	.723
7	.719

For this assumption .75 is a good null hypothesis for several dimensions.

Since issues vary in importance, the index should show the "effective" number of issues, and the natural approach is to reweight the issues in terms of issues of equal importance. The index used is the standard "numbers equivalent" inverse of the Herfindahl index from economics and antitrust (Jacquemin 1987, 50–53). The Herfindahl index, H, is the sum of the squares of the individual shares, s_i:

$$H = \sum_{i=1}^{n} s_i^2, \quad \text{with} \quad \sum_{i=1}^{n} s_i = 1.$$

For N equally important issues, $1/H = N$. Thus $1/H$ reweights the actual issues by their importance to the number of equal-sized issues.

Table 6. Calculation of Hypothetical Correct Classifications, Dimensions 2–7

Number of Dimensions	Number of Points	% Correct
Two	1	1.00
	2	.50
	1	2.00
Average		.75
Three	1	1.00
	3	.50
	3	.50
	1	1.00
Average		.625
Four	1	1.00
	4	.63
	6	.50
	4	.63
	1	1.00
Average		.625
Five	1	1.00
	5	.63
	10	.63
	10	.63
	5	.63
	1	1.00
Average		.648
Six	1	1.00
	6	.67
	15	.6
	20	.50
	15	.6
	6	.69
	1	1.00
Average		.595
Seven	1	1.00
	7	.69
	21	.69
	35	.50
	35	.50
	21	.69
	7	.69
	1	1.00
Average		.588

Note: The calculations by dimension weight each dimension equally.

Ranking issues by size, one possible distribution of issues has the ith issue's size, s_i, being a constant ratio β larger than the size of the $(i + 1)$th issue. For example, each issue might be 50% larger than the next, so $\beta = 1.5$. In general, $s_i = \beta s_{i+1}$ and $\beta > 1$. We can calculate β for some potential issues. Political issue dimensions might be related to consumption, industries, or occupations. Their size distributions could then approximate the size distribution of political issues. In addition, Table 7 shows the distribution of energy

Table 7. Relative Importance of Categories by Issue

Issues	No. of Categories	β
Consumption	12	1.51
Manufacturing industries	20	1.22
Industries	9	1.34
Occupations	16	1.28
Energy issues	9	1.32
House roll calls	15	1.41

Sources: U.S. Bureau of Census, *Statistical Abstract of the United States* (Washington: GPO, 1985); idem, *1980 Census* (Washington: GPO, 1980); Congressional Quarterly, "House Roll Calls," *Congressional Quarterly Almanac* (Washington: CQ, 1985); Peter Van Doren, "Limitations of Roll-Call Vote Analysis" (presented at APSA meeting, Washington, 1986).

Table 8. Adjustment of Correct Classification for Correlation

Correlation	Correct Classification	Percent Increase over 75%	Adjusted Average Correct Classification
.0	.75	0	31.33
.12	.77	6.2	26.75
.24	.78	12.5	21.52
.45	.81	25	8.44
.71	.88	50	—

Note: Correlation increases classification success from 75% in two dimensions approximately as the square of the correlation. For parallelograms with equal heights and lengths, these changes in correct classification were obtained. The last column shows the average Poole and Rosenthal average correct classification of 82.83% corrected for various degrees of correlation.

proposals in Congress across detailed issue categories and the distribution of all House roll call votes across *Congressional Quarterly*'s major issue categories.

We now calculate hypothetical "equivalent numbers" for specific βs for several finite numbers of categories also for an infinite series. For $\beta = 1.5$—and n issues —$1/H$ is

Number of Dimensions	$1/H$
2	1.9
3	2.7
4	3.4
5	3.8
6	4.2
7	4.5

For the infinite series $i = 1, \ldots \infty$, $1/H = (\beta + 1)/(\beta - 1)$, giving

β	$1/H$
1.2	11
1.25	9
1.33	7
1.4	6
1.45	5
2.0	3

Thus, even with a large or infinite number of dimensions that are not equally important, the *effective* number of dimensions is quite small—in the range of 3–7.

Real data are not likely to be perfectly orthogonal, as the null hypotheses have assumed. A more reasonable null hypothesis would be random draws from an underlying orthogonal distribution, which would mean that there would be some correlation in the data.

In Figure 3, even if legislators were distributed exactly uniformly in the space, random variations in attendance or perception errors would cause some deviation from a strictly uniform distribution. Similarly, if the cleavage of bills were determined by chance from a uniform distribution, it would be unlikely for them to have the completely orthogonal distribution shown in Figure 3 and assumed for the null hypothesis. Each of these would increase the correct classification level of the null hypothesis. If those correlations could be calculated, a higher and more precise null hypothesis correct classification would be available.[5] Table 8 has some exploratory results, showing that if substantial correlation occurs, it will overstate the importance of the first dimensions by a considerable degree.

A related null hypothesis problem exists in studies (e.g., Poole and Daniels 1985) that correlate votes with dimensions or voting scales like ADA rating. Such studies must be careful to avoid spurious correlations. For example, in Figure 2, there are two bills, one left-right and one up-down, with equal numbers of legislators in each quadrant. Suppose that A is considered left and is opposed to D, so A–D is the single dimension. The correlation between *A–D* and the votes on the two individual bills is .71, even though the two bills are orthogonal, so the correlation between votes on the left-right and up-down issues is zero. To see this, assign each quadrant x,y values: $A = -1,1$; $B = 1,1$; $C = -1,-1$; $D = 1,-1$. The *A–D* dimension then has location values given by $x - y$: $A = -2$, $B = 0$, $C = 0$, $D = 2$. The variance of the x dimension $= 4$, the variance of the *A–D* cleavage $= 8$, while the covariance of x and $A–D = 4$, so the correlation of x and $A–D = .71$. Thus, correlating an average of all votes with individual votes or other averages gives high correlations purely as an artifact. Thus Poole and Daniels' (1985) .81 correlation between ADA rating and the left-right dimension may not be so surprising.

Dimensional Findings Recalibrated

A natural way to evaluate dimensional findings is by their classification improvement over the null hypothesis. Such findings are often placed on a 0–1 scale. If the null hypothesis is 75% correctly classified, unidimensional unfolding should do better, measuring improvement as the percentage of the previously incorrectly classified votes that are now correctly classified. Thus, a reported 80% success rate explains 5 of the 25 points above the 75% null hypothesis success ratio, for an adjusted success rate of 20%. (Success below 75% would be evidence against the 75% null hypothesis.) Recalculations with null hypotheses of 75% and 66.7% are presented in Table 2.

Since unidimensional classification never has less than 66.7% success over a period of more than 60 years (Poole and Rosenthal 1985b, tbls. 1–4) for a variety of methods and issue domains, the 66.7% null hypothesis is not rejected. The expected number of points less than 75% is approximately 2 for their reported means and variances, so the absence of such points is not too unlikely. It is striking that all but two ratios are between 75% and 90%, which seems very unlikely for a variable with a range of 50%–100%.

For the 75% null hypothesis, the classification success of one dimension is substantial but not dominant; about a third of the votes above 75% are successfully classified. With Ladha's unidimensional model—based upon utility maximization with error—23%–27% are successfully classified. This is good performance for a single variable, given the many possible determinants of legislators' votes. But it leaves most of the votes to be explained by other factors or by random error. With the 66.7% null hypothesis a higher success level of around 50% is attained, but it is not so high that little remains unexplained, as the original numbers implied.

Table 3 recalibrates Poole and Daniels' (1985) reported classification success ratios and Table 4 reports the R-squareds from regressions of dimensions on interest group ratings. They use the same votes that led to the interest group ratings to find the dimensions. Since the interest groups choose votes based on the major cleavages in the Congress, I showed above that a fair null hypothesis is the success of one dimension fitted to two orthogonal dimensions with a zero correlation between them. It has a correlation of .71 with them, and so $R^2 = .5$, which is the null hypothesis for regressions in the tables.

For the classification results the second dimension is moderately important: 4%–11% of votes. Interestingly, the first dimension becomes less successful when high majority votes are eliminated, so the legislator's votes might affect policy. The regressions show an important first dimension, a moderate increase in R-squared from the second dimension, and a small increase from the third dimension. Since dimensions are invariably correlated, this overstates the relative success of the first and second dimensions. Table 3's results seem similar to those of Ladha and Poole and Rosenthal in Table 2; the regressions seem more favorable to unidimensionality.

Considering all of these findings, Poole and Rosenthal's (1985a) claim to have developed a superior technique for finding dimensions remains warranted: the dimension(s) have substantial explanatory power. However, the dimensions, particularly the first, are not nearly as powerful as they imply.

Conclusion: The Importance of *Some* Unidimensionality

If one stable dimension could "explain" most roll call voting, the interpretation of this result would remain a problem. Does it mean that the public's political concerns are really one-dimensional? Or does congressional coalition building reduce the number of dimensions?

Statistical consumer demand studies may give some perspective on this problem. Engel's law was discovered first: higher family income increases as the share of income spent on food falls—a one-dimensional analysis. Later, other dimensions were discovered: socioeconomic class, size of family, the urban-rural dimension, culture, price. And demand patterns vary across goods: Engel's law does not hold for steak.

For Engel curves to be straight lines—and so fit exactly—preferences must be homothetic: indifference curves must have the same shape for every level of income (Deaton and Muellbauer 1980, 145-58). That is very unlikely. Nevertheless, in the classic study of family budgets (Prais and Houthakker 1955) the pure linear form gives an average R-squared of .81 for the six major food categories (p. 95), while a regression analysis with many additional explanatory variables gives an R-squared of only .86 (p. 141). Yet demand analysis has properly concentrated on these additional variables as more fundamental explanatory factors.

Roll call analysis could develop in a similar manner. One dimension has been identified, but other dimensions have also been discovered and must be explained. And their relation to deeper underlying causes—constituency preferences, party coalitions, and legislative structure—must be investigated.

Marxist class analysis provides an analogy to the coalition structure of voting. Two classes conflict: "Which side are you on?" Yet some groups are not naturally on either side. *Analysis* of class conflict requires that we explain how these groups choose sides even if only two sides are seen in conflict. The legislative majority rule forces two sides on any vote; it may also force coherence in voting along dimensional alignments (Koford 1987, 1989).

I have shown that the classification success of dimensional analyses of legislative voting has been overstated. Null hypotheses show that one dimension explains one-quarter to one-half of roll call votes. Yet the unidimensional model's success is greater than would be expected if voting represented constituencies. The explanation may be found in the costs of organizing legislative coalitions, which concentrate bills and votes on the single most prominent dimension in the issue space.

Notes

An earlier version of this paper was presented at the 1986 APSA annual meeting, Washington. Comments from Howard Rosenthal, Keith Krehbiel, Jeffrey Miller, Jerrold Schneider, Jack Wright, and members of the USC/UCLA Microeconomics Workshop were very helpful.

1. However, Clausen (1973), Clausen and Cheney (1970), and Collie and Brady (1985) have found several independent dimensions. In her literature review, Collie (1985) regards this view as dominant.

2. William Schneider, "Borrowing from 10 Con-

gressional Rating Organizations," *National Journal,* 8 May 1982; idem, "Party Unity on Tax, Spending Issues," *National Journal,* 7 May 1983; idem, "Democrats, Republicans Move Further Apart on Most Issues," *National Journal,* 12 May 1984.

3. An analytically proven null hypothesis for scaling and unfolding has not been developed in the psychological literature. Instead, arbitrary success criteria (.9 in Guttman scaling) have been adopted (Torgerson 1958, chap. 12.4).

4. The amount of correlation that is likely depends upon the number of data points and the number of dimensions. A very large amount of data should be nearly orthogonal, while smaller amounts of data should have increased correlations. As correlation follows the F-distribution, it is possible to work out the distribution of correlation for two dimensions.

5. It would be useful to calculate classification success for the British Parliament as a comparison.

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CONTROVERSY

PRINCIPAL-AGENT MODELS OF POLITICAL CONTROL OF BUREAUCRACY

In the March 1988 issue of this Review, *B. Dan Wood invoked a principal-agent perspective to establish the importance of democratic hierarchies in shaping the outputs of public bureaucracies. He tested the model with air pollution enforcements of the Environmental Protection Agency (EPA) between 1977 and 1985. His results suggested some limitations on bureaucracies' responsiveness to elected political institutions, consistent with a principal-agent perspective with emphasis on the agent. Brian J. Cook challenges features of the design and empirical testing of Wood's hierarchical model. He questions the accuracy of Wood's characterization of the extent of EPA autonomy and power. Cook argues that a proper model requires recognition of the multiple principal nature of the U.S. system, and he advocates consideration of the normative foundations of principal-agent theory. Wood responds to both the substantive and methodological issues raised and suggests an eclectic approach in future research.*

The longitudinal analysis of the Envirnomental Protection Agency's (EPA's) Clean Air Act enforcement efforts reported by B. Dan Wood (1988) questions the efficacy of principal-agent models of bureaucratic policy choice and supports in their stead a model of bureaucratic policy-making autonomy. On factual, theoretical, and methodological grounds Wood's analysis is problematic, however, and thus does not undermine the principal-agency perspective to the extent the author claims.

Specifically, the analysis does not take sufficient account of the historical development of the EPA and how its character as an administrative organization has been shaped by the policy preferences of presidents *and* members of Congress. This leads to a deficiency in specifying the model subjected to empirical test. The model does not adequately operationalize the multiple principal nature of a "political-bureaucratic policy hierarchy."

Building an enhanced model will help forge closer links between formal models, quantitative analyses, and case studies that raise similar questions about political control of the bureaucracy. Establishing those links also reveals the need for direct, open debate on the normative underpinnings of democratic hierarchy models. The issue is whether administrative agencies ought to be understood as mere agents of chief executives and legislatures or as institutions with a distinctive representational role.

EPA's Organizational Character

Wood seeks "to test the adequacy of a principal-agent framework in explaining outputs from an agency that should . . . conform to the model of bureaucratic autonomy." He contends that "EPA provides a formidable test for the model since it traditionally has possessed all of the fea-

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tures which vest an agency with power" (p. 214). Yet Wood's portrait of a powerful, autonomous EPA leaves out important facts about the agency's founding and subsequent development. The EPA story reveals an agency that appears to fit the principal-agent framework, especially a model in which *multiple, competing* political principals attempt to control agency policy-making behavior, more closely than Wood's description suggests.

The EPA was founded, Wood explains, by an executive reorganization that consolidated 15 separate programs from at least three agencies. Moreover, Wood notes, EPA was not organized as a multiheaded commission outside the executive branch but as an independent agency in the executive establishment, headed by a single administrator appointed by the president (pp. 215–16). Yet Wood does not appear to recognize that this particular attribute of EPA organization is an important clue to EPA's political circumstances. As Harold Seidman (1980, 252) has observed, how an independent agency is structured determines the extent of its independence from political control. In the case of the EPA, the agency's internal structure reveals important *constraints* on its independence.

Wood's description of the agency's internal structure leaves the impression that the EPA was fully organized along functional, rather than programmatic lines: "The major subdivisions of EPA were divided into areas such as enforcement, planning, research and development, hazardous material, and management. Within these divisions were offices that dealt with the major programs to be administered by EPA" (p. 216). The saga of EPA's organizational development is rather more complicated, however. The White House reorganization plan featured the elimination of the new agency's programmatic inheritance through a three-stage transition to a fully functional, integrated organization. Yet the EPA's internal organization has largely been frozen at stage two of the plan, featuring three functional offices under assistant administrators (Planning and Management, Standards and Compliance, and Research and Monitoring) plus the individual programs under their own assistant administrators.

The primary obstacle to completion of the transition has been the long-standing conflict between the president and Congress over environmental policy objectives. As Marcus (1980b, 78) has explained, the White House's objective has been to manage the environment comprehensively and achieve efficiency in making pollution reductions. Congress's goal has been to make rapid progress with respect to specific pollution control problems with substantially less regard for cost.

The White House has attempted to impose its view of environmental policy on the agency through regulatory reform programs and management of the regulatory decision-making process. The Nixon administration attempted to direct policy making and implementation in the agency through its "quality of life review" of EPA regulations (Eads and Fix 1984, 46–50). EPA's first administrator, William Ruckleshaus, responded by using a "functional" unit—the Office of Planning and Management—to produce analyses to refute White House policy arguments. The irony in Ruckleshaus' action is that it created an organizational home in the EPA, currently called the Office of Policy Planning and Evaluation (OPPE), for economists and other analysts who actually tend to share the presidential perspective on environmental policy objectives.

Jimmy Carter exercised perhaps the greatest influence over the agency. Carter's external regulatory management process, the Regulatory Analysis Review Group, targeted numerous EPA regulations for review (Eads and Fix 1984, 54–65; White 1981). More important, his appointees to the agency expanded and invigorated the Office of Planning and Management. The office then launched a wide range of management and policy reforms, which included introducing economic incentive ideas into the design and implementation of air pollution control programs (Cook 1988). These reforms have had recognizable impacts on policy and program operations.

In addition to the use of political appointments, staff reductions, and internal reorganizations to influence EPA behavior, the Reagan administration employed yet another version of an external regulatory management process, operated by the Office of Management and Budget (OMB), which featured attempts to apply cost-benefit analysis to regulations proposed by a long list of agencies, with EPA getting special scrutiny (Eads and Fix 1984, chaps. 6, 7). Reagan appointees used OPPE to carry out presidential policy intentions to only a very limited extent because, at least early on, the office was bogged down by its effort to complete the Carter era reforms in the face of stiff initial resistance from administrator Ann Burford.

In contrast to presidential control techniques, Congress has attempted to place its stamp on environmental policy through the programmatic side of the organization. As Ackerman and Hassler (1981) have argued, the burst of regulatory statutes emanating from Congress in the 1970s reflected Congress's intention to go beyond the "New Deal model" of regulation in its approach to social regulation. Statutes designed under the new approach featured designation of specific standards and numerous agency-enforcing requirements. Congress thus attempted to expand its role in policy *implementation*, particularly in environmental policy; the Clean Air Act is the premier example.

The agents for Congress's policy intentions toward the environment have been the program and enforcement offices in the EPA. Congressional influence has been extensive, blocking, as already noted, any attempt to create a fully functional, integrated organization, and reinforcing the dominant professional ethos of attorneys and environmental engineers in the agency. Thus, the EPA readily fits the mold of agencies that "sit uncomfortably in a no-man's land between the 'president's men' and 'agencies of the Congress' and are considered fair game for both sides" (Seidman 1980, 60).

The Weaknesses in the Model

To establish a convincing empirical test of a formal model of agency behavior, it is crucial that both the institutional context and the internal character of the agency be thoroughly understood and effectively represented in the model. These model components are what Moe (1985) calls the exogenous variables and the endogenous core, respectively. For the EPA, its internal organization, and thus its implementation behavior, has been shaped significantly by actions initiated by both the president and Congress. By failing to recognize the structure and extent of this influence, the model Wood tests with data on air pollution enforcement takes insufficient account of either the exogenous or endogenous factors that may have influenced agency actions during the time period covered by the data.

At base, Wood's model lacks a variable representing Congress as an independent influence on EPA behavior. The single-model component representing Congress is the fiscal year 1982 budget reduction suffered by the agency. Wood interprets its impact as follows: "Executive authority was made effective through the power of the purse. That is, Congress mediated the relationship between the executive and the bureaucracy by giving its tacit approval to administration policies with large budget reductions" (p. 228). Apart from the rather dubious picture this description paints of Congress not as a politically (and constitutionally) independent principal but as merely a mediator between the president as principal and the EPA as agent, the use of budget decisions as exogenous variables raises a serious problem.

As Wood acknowledges in a footnote, the fiscal 1982 budget reduction is a less-than-perfect representation of Congress because the budget is an instrument for controlling bureau behavior that is shared by the president and Congress. Wood recognizes that how to separate out the independent influences of multiple principals that share means of control over a common agent is a problem of theory. The statistical solution he offers, however, along with his concept of "coarchy," does not get to the heart of the problem, it ignores it. Congress employs numerous instruments in addition to the budget to influence agency decision making (McCubbins and Schwartz 1984; Ripley and Franklin 1987, 68–84; Weingast 1984). Even when the body as a whole comes to agreement with the president on a budget initiative, substantial opposition may still exist in Congress. This opposition may then attempt to blunt the impact of a budget reduction through other means. Wood's reference to the frequent testimony of Reagan's EPA administrator Ann Burford and her deputies (pp. 218–19) is confirmation that Congress was attempting to shape EPA actions through means other than appropriations.

Wood's model thus does not capture the essence of Congress as principal in a democratic hierarchy. It represents Congress as a unified principal with a uniform character ("more Republican than at any time since the 1950s," p. 219), when in fact Congress is a collection not only of individuals but also of organizational subunits, where most of the legislative work is done. Because Congress does most of its work in committees and subcommittees, efforts at control of agency policy making are concentrated there.

Wood gets himself into particular trouble on this point in his rejection of what he calls the "congressional-responsiveness hypothesis." His analysis of EPA abatement activity (pp. 225–27) shows a sharp increase in abatement actions in the first month after Reagan's inauguration, a sharp decline following the fiscal 1982 budget reduction, a gradual increase up to the March 1983 Burford resignation, and then a more abrupt increase afterwards. Wood concludes that "the model suggests . . . clean air abatement actions were manipulated by EPA bureaucrats at crucial times to boost regulatory control" (p. 227).

Wood goes on to argue that "the congressional responsiveness hypothesis . . . does a poor job of explaining (these) variations in clean air outputs. At the time of the Reagan inauguration, when enforcements increased sharply, there was no stimulus sent by Congress or its committees to cause the increase" (p. 228). If history is any guide, however, EPA may have been under congressional scrutiny in early 1981, as it was in early 1977, when the Senate extensively reviewed EPA programs and debated future policy directions during the transition from the Ford to the Carter administration (U.S. Senate, Committee on Public Works, Subcommittee on Environmental Pollution, 1977).

Wood offers no support for his claim, which ideally would appear in the form of variables representing a purely congressional intervention that invoked little or no EPA response. Without such evidence, one may plausibly speculate that given the reauthorization battle for the Clean Air Act looming in 1982 and Reagan's antienvironmentalist predilections, environmentalist supporters in Congress sent signals through a variety of channels to EPA's air quality programs and enforcement offices. Thus, while the fiscal 1982 budget reduction sent a strong message to the agency, particular committees or subcommittees in Congress may have sent countervailing signals to which some EPA operatives readily responded even under circumstances of curtailed resources.

This point can be underscored with information on the differences in policy preferences across committees. For example, although the Senate appropriations subcommittee responsible for EPA funding was antienvironmentalist, as measured by League of Conservation Voter ratings of the individual members (Barone and Ujifusa 1981, 1983), the Senate's Environment and Public Works Committee and Environmental Pollution Subcommittee were chaired by strongly environmental Republican senators, Stafford of Vermont and Chaffee of Rhode Island, respectively.

These deficiencies in specifying the model appear to stem from an incomplete reading of the character of the endogenous core. Although nominally a functional office, EPA's Office of Enforcement has always been programmatically oriented. From the beginning it has been staffed by attorneys and environmental engineers, and its internal organization includes separate air and water program enforcement divisions. Most important, like the air programs office, EPA's air enforcement staff has always been strongly tied to congressional intentions regarding environmental policy. Hence EPA's enforcement bureaucrats have tended to be much more receptive to policy signals from congressional committees than from the president.

Recognizing the peculiar connection between the agency's internal structure and its external relationships reinforces the need for a broader representation of Congress in the model. It also sheds light on a problem for longitudinal analysis of the sort Wood has undertaken. Congressional committees and subcommittees do not attempt to influence agency behavior just through discrete "interventions." Use of such direct controls is infrequent, although regular in the case of appropriations. But committees also try to work their will by establishing a long-term pattern of interaction and communication with agency bureaucrats. It is difficult for Wood's sort of analysis to capture this form of congressional influence on agency behavior because no distinctive events are involved that can serve as variables in the model.

Building an Enhanced Model of a Democratic Hierarchy

Wood challenges arguments about the extent of political control of the bureaucracy growing out of the application of principal-agent theory to the U.S. system, contending that models of an electoral-administrative hierarchy do not "give sufficient weight to bureaucratic interests in shaping policy outputs" (p. 229). Weingast and Moran (1983), Bendor and Moe (1985), Bendor, Taylor, and Van Gaalen (1985, 1987), Moe (1985), Hammond, Hill, and Miller (1986), and Scholz and Wei (1986) have all attempted in one way or another to model formally the policy-making system and to test their models with computer simulations or real world data. The empirical analyses focusing on individual agencies in particular have generated results supporting a principal-agent perspective on political control of the bureaucracy. Does the choice of innovative methods explain Wood's challenging results? Or is it the choice of agency, thus reinforcing Seidman's argument that independence from political control varies not only across agencies but also across types of agencies (Seidman 1980, chap. 9, 10)?

Wood needs to do two things to help answer these questions: first, respecify the model by adding at least one variable representing a purely congressional intervention (one possibility is the House Energy and Commerce Committee Hearings on EPA administrator Burford's refusal to release documents on the Superfund Program to the committee—better still would be hearings held specifically on EPA enforcement activity); second, build a more elaborate model that more fully represents the president and Congress as distinctive political principals trying to control bureaucratic agents.

One possible approach would begin by expanding the time series backward, if the data are available. This would allow the data to include more than one change in administration, which in turn would provide a broader indication of the influence that changes in administrations—as a surrogate measure of the president's appointment powers—have on EPA behavior. Then one could introduce other model terms representing presidential control. Because with regulatory agencies the last four presidents have employed similar control techniques, variables such as the percentage of agency actions subjected to a centralized review process and the percentage of actions revised or rejected as the result of review may serve to measure further the extent of presidential control. A budgetary variable—for instance, a term representing OMB—might also be appropriate.

Next, one could introduce additional model terms to represent congressional influence. A budgetary variable is still required, but perhaps one more closely associated with appropriations subcommittee influence, like the one used by Weingast and Moran (1983, 787), is appropriate. Representation of authorization committee influence is also needed, however. Hence, following the leads of Weingast and Moran (1983) and Moe (1985), ideological indexes of the relevant subcommittees and committees and their respective chairs, and of the whole House and Senate, are important variables to add to the model.

A model built along these lines is probably more conducive to analysis by pooled regression methods than by time series impact assessment. Moreover, building such elaborate models comes with a price. Formal models will lose rigor and simplicity as they incorporate more of the details of policy making in the U.S. system. Paying that price by tapping the valuable information in case studies and broader analyses of bureaucratic politics seems imperative, however, if we are ever to advance formal theory beyond simplistic representations of institutional relationships in policy making.

Strengthening the connection between the "substantive complexity" (Moe 1985, 1115) of policy making and the conceptualization of that complexity in formal models poses a fundamental normative challenge to formal theory, however. Implicit in much of the principal-agent perspective is the notion that agents ought to be subject to control by principals. If agents were not under such control, they would shirk their contractual responsibilities and pursue their own ends, at direct cost to their principals. At lease in part, then, analysis should aid principals in finding effective techniques of control. With his references to the EPA as an organization that "literally represented the environment" (p. 216) and to bureaus as "agents of the law" (p. 231), Wood is challenging that notion as it has been applied to the U.S. constitutional system and the place of administrative agencies in that system (also see Aberbach and Rockman 1988). The issue Wood raises deserves consideration. Boiled down to its essence, the question is this: Why should public bureaucracies not be considered another form of political representation, with their status as agents of popularly elected executives and legislatures the subject of debate rather than the accepted point of departure for theory building?

BRIAN J. COOK

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Brian Cook makes some interesting arguments about my study of how EPA's clean air division responded to changing political conditions in the early Reagan administration (Wood 1988). Our controversy centers on two issues, one substantive and the other methodological. The substantive issue concerns the efficacy of standard principal-agent models for explaining relations between U.S. political institutions and public bureaucracies. The methodological issue concerns the narrower question of how to construct empirical tests of such models properly. On both matters I previously expressed reservations about existing approaches, including those Cook apparently advocates. Let me first address the substantive issue and then some of the methodological questions involved.

Bureaucrats: Passive or Strategic Actors?

My time series analysis of EPA clean air enforcements actually challenged two theories: the emerging principal-agent framework and the older strategic actor conceptualization of bureaucracy. Neither approach offers a satisfactory explanation of political-bureaucratic relations. The clean air research should inspire revision in both.

Cook directs his critique only to the principal-agent side. He argues that the model in the clean air piece contains no distinctive representation of each principal in EPA's policy system. He thinks this is problematic because multiple principals were important in the evolution of EPA to its modern form (see also Jones 1975; Marcus 1980a; Melnick 1983). According to Cook, a proper test of the hierarchical model requires a multivariate specification to account for the rich political context in which EPA operates.

To the charge of modeling the EPA response to events, not actors, I readily admit. A rationale for this, of course, is that in the U.S. system constitutional actors share some tools for controlling bureaucracy (i.e., budgets, appointments, lawmaking authority). It makes little sense, therefore, to attribute stimuli to single actors when there is no way to separate their influence.

This issue aside, the evidence for hierarchy remains incomplete, whether one considers events or actors. Enforcement activity in the early Reagan administration moved at times with direction and magnitude not readily predicted by the stimuli. These predictive failures result from what I perceive as a defect in applying standard principal-agent models to political-bureaucratic relations. What, specifically, is the nature of that defect? Principal-agent models are grounded in economics and make assumptions that do not extrapolate precisely to the problem of bureaucratic control. Their original context is the business environment where managers seek to motivate workers who want to minimize effort (see, e.g., Kaplan 1984; Ross 1973; Spence 1974). Accordingly, principal-agent models assume bureaucrats are passive, lazy, and calculative only to the extent they want to avoid work. The overarching issue addressed in the clean air research is whether passive actor assumptions fit for U.S. bureaucracies, especially those with independent attributes. Abundant case study evidence confirms that EPA has independent attributes, Cook's argument notwithstanding (see Ackerman and Hassler 1981, 1; Marcus 1980a, 269–303; Regens and Rycroft 1988, 115–17).

The clean air model reveals that passive actor assumptions are inappropriate. EPA bureaucrats engaged in strategic manipulation at crucial times to move outputs in unanticipated directions. They acted independently and sometimes in opposition to political principals to maintain established policy. The findings, of course, are consistent with well-known anecdotal evidence about how EPA careerists conducted a guerrilla action with leaks to the

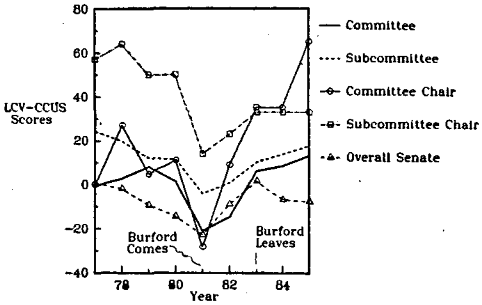

Figure 1. Senate Environmentalism, 1977-85

media and Congress to rid themselves of the Burford regime. They are also compatible with other political research suggesting bureaucrats have both the capacity and motivation to affect public policy outputs (Downs 1967; Heclo 1977; Kaufman 1981; Meier 1987; Mosher 1982; Niskanen 1971, 1975; Peltzman 1976; Rourke 1984; Stigler 1971; Wilson 1980). Still, Cook argues there must be another explanation for the movements.

The explanation he proposes is congressional responsiveness. Cook speculates that Congress was somehow in control of EPA's clean air activities through personnel in the program and enforcement offices. The argument is made by assertion and disregards the massive personnel changes and reorganizations early in the Reagan administration (Andrews 1984; Gottron 1982). Furthermore, all of Cook's evidence for congressional responsiveness is vague. He claims that "strongly environmental" Republican senators, Stafford of Vermont and Chaffee of Rhode Island, chaired Senate committees responsible for overseeing EPA programs. He also says that EPA "may have" been under congressional scrutiny in early 1981, as it was in early 1977, when the Senate considered clean air amendments. Finally, he adds that particular committees or subcommittees in Congress "may have" sent countervailing signals to which some EPA operatives responded despite curtailed resources. Interestingly, Cook's theme that EPA responded to Congress is "fraught with innuendo about strongly environmental" Republicans and "may haves" but offers no real evidence. Why such a soft approach when data are available? The data do not support Cook's hypothesis.

Historically, the Senate was progenitor and monitor of most environmental programs. Figure 1 contains time series of Senate environmentalism for years 1977-85. The statistics shown are the difference in mean League of Conservation Voters (LCV) score and U.S. Chamber of Commerce (CCUS) score for relevant Senate entities (Barone and Ujifusa 1977-85). The actual voting records of members of Congress for selected environmental and business issues comprise the measure. I report scores for the chair and membership of the Senate Committee on the Environment and Public Works, the chair and membership of the Senate Subcommittee on Environmental Pollution, and the entire Senate. Note that both committee and subcommittee chairs (Senators Stafford and Chaffee) were less supportive of the environment in 1981 than their Democratic predecessors and by a large margin. Oversight committee members were also less environmentalist. Furthermore, overall Senate environmentalism declined in 1981.[1] Reduced support for EPA remained below that of the previous Democratic administration through 1982, demonstrating no strong stimulus for increased enforcement before 1983. Even then, the timing of the March 1983 increase reported in the clean air piece reflects more a celebration by bureaucrats of changing EPA leadership than a response specific to Congress.

The voting records of legislative principals between 1981 and 1983 do not lend credence to a congressional responsiveness hypothesis. Specific actions taken by

Congress also cast doubt on Cook's explanation. Senate confirmation hearings resulted in easy approval for EPA leadership who had obvious hostility toward previous agency policy (U.S. Senate 1981a). Appropriations committees readily approved large budget reductions after fiscal year 1982 for EPA. Furthermore, as Cook probably knows, oversight hearings did not commence in either house of Congress for a full nine months after the Reagan inauguration (U.S. Senate 1981b). A reading of those hearings reveals conflict on the Republican-controlled committee and no clear mandate to EPA.

One could reasonably argue that the post-1980 Congress did not get religion on environmental issues until well into 1982. This interpretation is consistent with the Senate voting records reported in Figure 1. It is also consistent with some case study evidence (see Mitchell 1984). In March 1982, information surfaced indicating that "the environmental vote alone, if fully mobilized, could make a swing of 11 points in [the 1982] elections for the House and Senate" (Harris 1982). The strong salience of environmental issues and the increased activity of environmental groups gave some members of Congress incentive to reconsider their position. One might surmise that this caused the increased Senate environmentalism observed in Figure 1 and demonstrates the overriding importance of constituency to political-bureaucratic relations. Significantly, this explanation reverses the hierarchical model by suggesting that strategic bureaucrats can manipulate Congress through mobilized support.

Additional evidence on EPA and Congress early in the Reagan administration comes from informal elite interviews I conducted in the summer and fall of 1987 (reported in Wood 1987). Those interviews asked several pertinent questions of career bureaucrats who were at EPA through this period: "How did relations with Congress affect these matters? Was there pressure from the Committee on the Environment and Public Works to increase outputs? What about the House of Representatives? Can you offer any insights as to what mechanisms were responsible for the increases which occurred?" In none of 27 interviews did a respondent provide evidence of congressional intervention. In fact, the consensus was that a lack of executive *and* congressional support caused a severe decline in morale at EPA.

So why did some EPA outputs change in unpredicted directions between January 1981 and March 1983? Respondents mentioned several factors as contributing. For one thing, EPA had slack resources before fiscal year 1982 because the Carter budget was the largest in agency history. There were delays in filling some important positions in regional offices. Administrative constraints were also important. When asked what caused the upward movements in enforcement activity during a conservative Republican administration, most respondents answered, "We did our job, regardless." Participants were highly professional, obviously concerned for career, programs, and the organization. Interestingly, they suggested that the increased enforcements did not originate from program or enforcement offices, as Cook suggests, but from the organization base. Regional office personnel stepped up the frequency of monitoring and abatement activities independent of overhead administrative direction. These were personnel who had never appeared in a congressional hearing or communicated with members of congress in any way. So it hardly seems likely that their activities were part of a long-term pattern of "interaction and communications" with Congress.[2]

Testing Principal-Agent Models

My research on EPA's clean air program tested the assumptions underlying application of principal-agent models to political-bureaucratic relations. It also improved methodology for testing them by isolating some specifics of how they operate. The clean air study strengthened the causal chain between actions taken by political principals and subsequent responses by bureaucracy. More directly, it showed that budgets and political leadership are important causes of organizational change. Principal-agent theory embodies a behavioral perspective that posits explicit stimulus-response relationships. Changes in political leadership, budgets, legislation, organization, oversight intensity, and directives should alter bureaucratic behavior. These are all stimuli evoking potential responses amenable to quasi-experimental evaluation.

Now, previous empirical tests of the hierarchical model were seminal because they transformed scholarly thought on political control of the bureaucracy (Chubb 1985; Moe 1982, 1985; Scholz and Wei 1986; Weingast and Moran 1983). Some of these studies employed "fully specified" models of the sort Cook says should be in the clean air analysis.[3] They found policy variations consistent with the orientations of political principals, but they isolated no specific stimuli to produce the responses. Moe (1985, 1107) hypothesized effects beginning with a president's first appointment, while expressly ignoring effects from budgets, presidential direction, organization, and subsequent appointments. Other studies considered executive and congressional ideology but made little effort to discern their modes of influence. They showed covariation, not causal movements. Yet there is no reason why appropriate methods cannot isolate the mechanisms of political-bureaucratic control.

Time series impact assessment is well suited to this task, but there are design issues to address for research to be persuasive. First, measurement must become more sophisticated. The single binary dummy variable used in past research to represent presidential orientations is inadequate. It unrealistically assumes a stimulus that changes after the election year to remain constant for the presidency. The theoretical rationale for assuming this functional form over temporary or complex effects is weak. For example, note that the presidential intervention in the clean air study evoked a temporary response, lasting only about 23 months. Additionally, single binary dummy variables can obscure multiple stimuli. Consider the first year of a presidency when some causal events are congressional. Under these conditions specification of a single presidential dummy includes effects that are not even presidential.

Similarly, researchers' previous rationale for using annual interest group support scores to represent Congress is also tenuous. These scores do not reflect specific stimuli and may bear little resemblance to actual events. Of course, Cook's notion that change in bureaucratic behavior is not always in response to stimulus is interesting. He argues that politicians manipulate bureaucracy by remote control through a pattern of enduring relations. However, interest group support scores do not measure such patterns either. They measure the annual voting record of members of congress on selected issues, not congressional-bureaucratic interactions. Moreover, Cook should note that such concepts are not amenable to empirical proof since no measure exists (or will likely exist) of such abstract connections.

A second issue in designing empirical tests of principal-agent models for political-bureaucratic relations involves the problem of collinearity. Many researchers are familiar with collinearity as a statistical issue, but it also requires consideration in research design. As Campbell and Stanley (1966, 39) observe, "Failure to control history is the most definite weakness of [time series analysis]. That is, the rival hypothesis exists that not X but some more or less simultaneous event produced the shift." For example, note in Figure 1 that several independent variables Cook says should be in the clean air analysis are strongly collinear: overall congressional ideology, committee ideology, committee leadership ideology, and Administrator Burford's tenure. This situation is also common for other policies. The design question is how to separate causal from spurious or coincidental effects. How does one know which of the effects is responsible for change in implementation outputs or which combination is responsible? More problematic, how does one know that some exogenous process (i.e., constituency) is not driving all movements? These issues are especially pernicious when one considers that measurement error exists. Under these conditions multivariate methods (pooled or otherwise) do not provide a magic formula for revealing the truth. Clearly, the solution is not including all interesting variables in a single-equation analysis, as Cook suggests. The analyst should select variables carefully, based on sound theory. Even then, results should be reported in the context of obvious limitations from collinearity.

A final concern in designing time series research for evaluating political-bureaucratic relations concerns the time intervals of the data. Showing causal relations requires data divided by sufficiently small intervals to establish sequence. The clean air study revealed that annual data are troublesome because they obscure the underlying dynamic of a process. Also, annual data do not permit establishing stimulus-response outcomes because of multiple effects in the same year. So it may not be possible to demonstrate causal movements with such data.

On the other hand, time series with finely divided data, as in the clean air study, can also be problematic. They are not conducive to the sort of multivariate analysis Cook desires because there are no good monthly measures of executive, congressional, or judicial orientations. Still, causal analysis of events and subsequent responses provides valuable information about why bureaucracy responds to politicians. A single study obviously does not tell all, but the methodology holds the promise of revealing much more.

What Next?

It is important to both theory and practice for social science to isolate specific mechanisms of political control of the bureaucracy. Research should uncover when, how much, and why public bureaucracies respond to political stimuli. Toward this end, all sorts of methodologies can be useful. Carefully specified multivariate models can reveal coarse tendencies of a policy process. Quasi-experimental time series designs can describe the underlying dynamic and fill in the details. Inductive theory building should not culminate before doing more analyses of both types.

Future studies should focus on a variety of administrative forms to identify the features that produce variations in responsiveness and stability. The EPA has extreme institutional characteristics conducive to stable equilibrium. New research may reveal another extreme, where the agency responds limply to efforts at political manipulation. Social science should map the permutations of stability and responsiveness before confidently advocating a theory of democratic control.

Inductive analysis may result in modifications to existing theory. The clean air study suggests one such possible modification. Not all bureaucracies are passive in a democratic hierarchy. Some are fully endowed strategic players capable of maintaining an equilibrium with abundant energy.[4] Sometimes they may even move it in directions completely opposite from expectations of political principals. Furthermore, they rightfully have such capabilities.

Political principals create bureaucracies on behalf of democratic coalitions. They vest bureaucracy with a charge that is a sort of contractual agreement. The contractual agreement is between a static political coalition at one point in time and agents obliged to honor the agreement at future points in time. Agents are contractually bound to the original agreement regardless of the status of the original coalition. New coalitions will, of course, develop and these have interests that may conflict with the established course of bureaucracy. These new coalitions are free to make demands through elected institutions. Elected institutions are also free to attempt shaping the behavior of bureaucracy. Still, until law changes, bureaucracy reflects the original coalition. Responsiveness should occur only in the limited context of boundaries established by the original contract.

Research shows that bureaucratic agents do indeed respond to change initiatives by contemporaneous principals. Nevertheless, their primary disposition is in following established interests. The fully institutionalized bureaucracy will maintain the organizational mission in preference to divergent political demands. The mechanisms of institutionalization are constituency, professionalism, leadership, routinization, esprit, and administrative constraint (Heffron and McFeeley 1983; Rourke 1984). Coincident with institutionalization is countervailing power to resist overtures from political principals.

Viewed in this light the task of future research involves two questions, one normative and the other empirical. The normative question concerns how stable and responsive bureaucracies should be. Some demand insulation from political influence for successful implementation (e.g., civil rights bureaucracies). Others require a more permeable arrangement (e.g., national security agencies). The empirical question involves how to design bureaucracies with an optimal blend of responsiveness and stability. Social science should isolate qualities of bureaucracy conducive to these features. With such information decision makers will ultimately be able to design and control bureaucracies rationally, rather than in the ad hoc style of the past.

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Notes

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1. Cook reports above that appropriations committees were also less environmentalist.

2. For clarity, the point here is not that bureaucracies are unresponsive to congressional oversight committees. Sometimes they are, but in this case congressional responsiveness explains movements poorly.

3. The term *fully specified* should not produce the conclusion that the Box-Tiao model in the clean air study suffered from specification error. Box-Tiao models include an ARIMA noise term to control for omitted variables. It is also common practice to overmodel, with additional intervention terms to improve model fit. Congressional variables were in several of the alternative specifications. The best model is the one reported and includes only relevant variables. An ocular test of the graphs reported in the clean air piece confirms the goodness of fit.

4. Note that strategic interaction between politicians and bureaucrats is consistent with the dynamic model reported by Bendor and Moe (1985). See also Bendor, Taylor, and Van Gaalen (1985) and Kamlet and Levinthal (1988).

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REVIEW ESSAY

MISTRUST BUT VERIFY: MEMOIRS OF THE REAGAN ERA

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Revolution. By Martin Anderson. San Diego: Harcourt Brace, Jovanovich, 1988. 486p. $19.95.

Behind the Scenes. By Michael K. Deaver with Mickey Herskowitz. New York: William Morrow, 1987. 272p. $17.95.

Reaganomics: An Insider's Account of the Policies and the People. By William A. Niskanen. New York: Oxford University Press, 1988. 363p. $22.95.

For the Record: From Wall Street to Washington. By Donald T. Regan. San Diego: Harcourt, Brace, Jovanovich. 1988. 397p. $21.95.

Speaking Out: Inside the Reagan White House. By Larry Speakes with Robert Pack. New York: Charles Scribner's Sons, 1988. 322p. $19.95.

The Triumph of Politics: Why the Reagan Revolution Failed. By David A. Stockman. New York: Harper & Row, 1986. 422p. $21.95.

In responding to a student question following one of his last public addresses as president, Ronald Reagan said that he might write a book after leaving office. Later, he signed a contract and presumably is at work. Alas, it may be too late. As George Eliot observed, "All biography diminishes in interest when the subject has won celebrity. . . . But autobiography at least saves a man or woman that the world is curious about from the publication of a string of mistakes called 'Memoirs'." The memoirs are already published—with the promise of more to come. That leaves a lot for the president to correct, and accuracy in recounting the facts has not been a Reagan strength.

Six recent books on the Reagan administration are reviewed here. They properly fit the category of memoirs, though the books by Regan, Speakes, and Stockman contain autobiographical sketches. These three books were written to sell. They are popularly referred to as "kiss-and-tell" books. "Hiss-and-sell" may actually be a more apt description. They tend to be "look-at-me" books, possibly fearing oblivion. No doubt Michael Deaver also wanted to sell his book. But his approach is different, somewhat more "look-at-the-president" in form.

The other two books were written primarily to inform. Anderson and Niskanen are economists, moved by their experiences in the Reagan administration to tell us how things worked. Anderson gained wide experience in policy politics by working for three Republican presidents. Not surprisingly his book is the most valuable of the six for political scientists.

What is the utility of memoirs? Properly done they can contribute (1) by providing previously unknown details of important political and policy events; (2) by revealing the strategic thinking of an administration; (3) by illustrating the processes of interaction among policy makers, within the White House and outside; and (4)

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by offering perspectives on the president and other major public figures that when taken as a whole, may form convincing portraits different from those in the media.

These are impressive contributions, but bear in mind that they may not be the goals of the authors. Only two of the authors have scholarly credentials and therefore could be expected to set academic- or research-oriented goals. For the others the stated goal is that of setting the record straight, typically in personal terms. For Deaver it was "to reveal Ronald Reagan as I knew him: the most human of American presidents" (p. 12). For Regan it was "to describe the President I served, the people I encountered in the course of that service, and the events in which I participated, exactly as I remember them" (p. xiii). Speakes had a wholly personal goal in mind, to provide "my account . . . of what went on in the Reagan White House and what my role was in those events" (p. ix). Stockman leaves us to speculate about his purpose, saying nothing directly. His prologue provides clues, however. He begins with the story of his trip to the woodshed following the publication of an article by William Grieder in the *Atlantic Monthly* that was based largely on conversations with Stockman. "The President's eyes were moist. It was unmistakable—they glistened" (p. 1). Stockman recounts that he rambled on to "what amounted to a capsule of my life story" (p. 1). The book is one of the more self-absorbed memoirs in this group, and that is saying a lot.

In contrast, the two scholar-policy advisers, Anderson and Niskanen, set forth clear statements of purpose touching several of the points made above. However far they may be from their academic training and experience, scholars still see a book as far more than printed pages between hard (or soft) covers. Anderson explains,

> This book is primarily a story about Ronald Reagan's rise to power in the United States, on what kind of man he is, the public policies he thought were important, and the main consequences of those policies. There is special emphasis on the worldwide intellectual movement that made Reagan's rise possible—the growth of the new capitalism. . . .
>
> This book is a story about the power of ideas, about how ideas come out of the intellectual world, are transformed by the world of presidential campaigns into items on the national policy agenda, and then how those ideas became law in Washington, D.C., and govern us and affect our daily lives (p. xxi).

Note that Anderson identifies all of the elements noted above: political and policy events, strategies, processes, and people. Niskanen gives less attention to people, more to policy, but again offers the recommended statement of intentions: "My primary objectives in this book are to explain the origin of Reaganomics, what policy changes were proposed and implemented during the Reagan administration, the relation between changes in policy and changes in the economy, and the probable future of Reaganomics" (p. vii).

What are the more specific contributions of these books for political scientists? The books by Anderson and Niskanen deserve a reading by all serious students of national politics and policy making. The others vary in their usefulness for scholars. For those books written with—or told to—a professional writer, it is questionable whether an accounting of an event is as it was remembered or as it was improved by the writer. Then there is the problem of the revelation—that inside tidbit that will make the newspapers and guarantee the breakfast television interviews (e.g., astrology in the White House for Regan, putting words in the president's mouth for Speakes). Scholars can ignore market-motivated exaggerations. But doubts are raised about the authenticity of other accounts. *Mistrust but verify;* that is, be skeptical but don't dismiss these memoirs entirely. Collected and compared, the individual perspectives—like the experiences of the proverbial blind men—may actually produce an elephant. I will discuss the books in pairs from the least to the most useful for the scholar: Speakes and Deaver, Regan and Stockman, Anderson and Niskanen.

Raymond Wolfinger once informed me that the plural of *anecdote* is *data.* The Speakes and Deaver books belie his assertion. For them, the plural of *anecdote* is *gossip.* Both books are filled with inside, personal stories—"backdoor at the White House" tales. The stories actually make for fun reading at times; their writers are quite good. But for the scholar, these books are best read from the index, checking on whether certain topics are included.

In *Speaking Out,* the political scholar first learns a lesson about White House personnel. And the lesson has to do with how a man of modest talents and accomplishments parlays staff-level contacts into a Washington career. Working for Senator James Eastland (D-Mississippi), Speakes got a job as a page for Ken Clawson's son. (Clawson was then Nixon's director of communications.) In return, Clawson arranged for Speakes to be interviewed for a job in the White House. "Nixon was clearly going down the tubes, but I said to myself, 'Having the White House on my resume will be worth whatever it costs' " (p. 38). Speakes stayed on with Ford, then through connections with a Reagan transition staff person, he met James Brady and returned once more to the White House. There are thousands of such hangers-on in Washington and we need to know more about them.

Speakes's book provides details on the personal infighting within the White House and between the White House and the press. Again, we only have Speakes's version, but apparently even tragedy, like that of the shooting of the president and Jim Brady, was the occasion for self-promotion. Only the most hardened cynic can fail to be disturbed by the pettiness of the actions described (e.g., lowering the podium for six-foot-four-inch David Gergen so that he would "tower over it like Ichabod Crane" [p. 244]). Infighting is common to all organizational life. Comparisons between the political (as within the White House) and bureaucratic versions should prove valuable in terms both of the functions of such behavior and of how it is accommodated.

In his description of the major issues with which he had to deal, Speakes chooses personal matters regarding the president (e.g., his health) and international events (Grenada, terrorist incidents, several involving the Soviet Union, the Philippines' crisis, Iran-contra). These examples tell us a lot about what makes news for the White House press corps. Complex domestic issues can be covered within the departments and agencies. Still it is worth noting that such topics as the budget, deficits, tax reform, trade imbalances, and Reaganomics barely receive mention (only in a brief summary "assessing Ronald Reagan"). Speakes asserts that "the economy is the best measure of the success of the Reagan Revolution" (p. 301), yet he has little to say about it. We know that domestic policy topics are raised at press briefings. Therefore we don't even have the full feel of Speakes's portion of the elephant, only a few of the touches.

Michael Deaver's "behind-the-scenes" account is actually more disjointed and selective than that of Speakes. His subjects are Ronald and Nancy Reagan, roughly in a two-to-one ratio (measured by index entries). The book is a remarkable display of devotion from a talented sycophant: "I never really worked for anyone except Ronald Reagan, after my one year as a sales trainee for IBM. . . . There is no question that my family felt they rated somewhere behind the Reagans in the long and sometimes bumpy ride from Sacramento to Washington. . . . It grew harder each year to break away from the kind of bond I believe we had forged. After a while, I no longer wanted to break away" (pp. 193-4).

Here is a hanger-on of a different type, attached not to the White House but to the "great man" and his family. Such persons also merit study. In fact, an analysis of what happens when the two types of hangers-on interact would be fascinating.

Deaver was good at his job, which he describes as "the staging of a media event; blending the gifts of Ronald Reagan with the proper pageantry" (p. 179). He acknowledges that "not everyone looked on this capacity with respect" (p. 170), but there is no denying its importance to the politics of the high-tech, media age.

Deaver's stories are about situations and people more than issues: the campaigns; Nancy's character and influence; relationships within the troika of Deaver, Meese, and Baker; managing the press; and, of course, the visit to the Bitburg cemetery (for which Deaver takes full blame). Scholars of the press and politics like Stephen Hess, Doris Graber, Michael Robinson, Thomas Patterson, Michael Grossman, and Martha Kumar will find nice illustrations here, most of which will confirm what they already know.

The books by Regan and Stockman provide a great deal that is of interest to students of politics and policy. Verification is essential, to be sure, but their treatments are more systematic than the two just discussed. There are also differences between the two books in this set. Regan, the man from Wall Street, tried to understand the politics of the Reagan era; Stockman, the man from Washington, resisted. Both were overcome with their status and perplexed by the president's style. But for all of his arrogance and self-confidence, Regan at least understood that he worked for the president, not the other way around.

It is unfortunate that Regan's bitterness toward Nancy Reagan, justified as it may have been, encouraged him to reveal her preoccupation with astrology. Royalty has always engaged in quirky behavior; yet our appetite for such revelations is never sated, so that that relatively small part of the book came to mark its publication.

The real contributions for scholars, however, are substantive and behavioral. For the first, Regan offers his recounting of tax policy decisions. Given other accounts—those by Anderson, Stockman (for the tax cuts), Niskanen, and Jeffrey Birnbaum and Alan Murray (*Showdown at Gucci Gulch)*—we should be able to get that story straight. For the second—the behavioral—contribution, Regan provides a fascinating study of contrasting executive styles—his and the president's. This more interesting set of lessons is not the subject of any one chapter. Rather it pervades the book, including even Regan's autobiographical chapters. This corporate executive entered what he describes as the "guesswork presidency": "From the first day to the last at Treasury, I was flying by the seat of my pants. The President never told me what he believed or what he wanted to accomplish in the field of economics. I had to figure these things out like any other American, by studying his speeches and reading the newspapers" (p. 142). Later when Regan took over as chief of staff he prepared a document to set the course of action for the second term:

> I was preparing a plan of action to the chief executive officer of the firm. According to the methods under which I had always operated, objectives were hammered out through discussion by the people who would have to carry them out, this consensus was reduced to writing, and the result was submitted to the man in charge. He then told you what he liked about the plan and what he did not like. You revised on the basis of his criticisms until you had a paper that he was willing to approve. Once he had signed off, debate ceased and you went to work to carry out the final plan (p. 266).

President Reagan read the paper at the ranch and handed it back to Regan "without spoken or written comment."

> "What did you think of it?" I asked.
> "It's good," the President replied, nodding in approval. "It's really good, Don" (p. 267).

The memo referred to was not minor. It offered a plan for the "maintenance of economic recovery, foreign policy, and the legislative agenda" (p. 265). The recommendations were "logical extensions of existing policy" (p. 266). Regan wanted approval through discussion. What he got was consistent with Reagan's style, as Regan tried to understand that style: "He trusted his lieutenants to act on his intentions, rather than on his spoken instructions. . . . Never did he issue a direct order, although I, at least, sometimes devoutly wished that he would" (p. 268).

One sentence in Regan's book demonstrates the differences between himself and Stockman: "Common sense suggested that the President knew something that the rest of us did not know. It was my clear duty to do things his way" (p. 268). Stockman drew no such conclusions. Reagan represented something bigger than Reagan and it was Stockman's task to see that this larger concept—Reaganism—was realized.

It is difficult to characterize Stockman's book in a few words. It is at once a confessional and a paean to his own brilliance. By his account, the embattled budget director understood Reaganism better than anyone else, including the president. Reagan's style set perfect conditions for this ambitious, self-described ideologue: "He gave us no orders, no commands; asked for no information; expressed no urgency. . . . We should simply get on with the job" (p. 76). Since Stockman had a "grand doctrine," he interpreted the president's "quiet message of confidence" as a mandate. "It would be up to me to design the Reagan Revolution" (p. 75).

Scholars will profit from reading the detailed story of the 1981 budget and tax policy battles. But they will need to meld this version with other versions—mistrust but verify. The story and Stockman's role in it are notable because it is so seldom that a president can credibly act on a mandate. So the budget director, the self-appointed designer of the revolution, had the advantage of an exceptionally popular president. Unless I missed it, this important point was not mentioned. In other words, the Reagan revolution, so-called, began with the election of Ronald Reagan and the success of congressional Republicans; not with the appointment of a budget director.

At one point, Stockman characterizes James Baker and Richard Darman as operating on the "governing principle": "Their idea was to achieve as much of the supply-side policy as you could without sacrificing the President's continuing capacity to succeed" (p. 235). Donald Regan, says Stockman, operated on the "echo principle": "Whatever the President insisted on, he would try to get—without regard to the price. He had been a Marine, and this was the moral equivalent of taking a hill" (p. 235).

By contrast, Stockman identified his vantage point as "ideological." Early in the book he outlines his "odyssey of an ideologue," expressing his admiration for James MacGregor Burns, E. E. Schattschneider, and Theodore Lowi. *The End of Liberalism*, in particular, greatly impressed him as descriptive of the parochialism of the Washington policy process. He was bound to take advantage of the opportunity provided by Reagan's victory to break the hold of a politics dominated by iron triangles.

Thus, "the triumph of politics" is Stockman's measure of failure. Throughout the book he poses himself as fighting the politicians and losing—an almost Carter-like struggle. At one point he notes that "democracy defeated the doctrine" (p. 204), but the sentiment is the same—politics won, ideology lost. Curiously, his determination to do what was right led to a kind of "splurge and purge" behavior as he leaked a version of the administration's intention to William Greider of the *Washington Post.* Later he acknowledged engaging in "out-and-out subversion . . . to force a tax hike" (p. 376). So here was a third hanger-on in the Reagan group—one devoted to a doctrine.

The last pair of books require less commentary, since their value to scholars is more obvious. As professional economists, Anderson and Niskanen are sensitive to the need to present reasonably objective accounts. That is not to say they are nonpartisan. Both were committed to the Reagan program. But an effort is made to analyze as well as justify.

There are differences between the books. Anderson was more the insider and so his account is more personal and he provides more description of the process. He was also involved in a wider range of issues, so his book is more comprehensive than Niskanen's (who concentrates on economic policy). Niskanen's role was limited to service on the original Reagan economic team and later on the Council of Economic Advisers.

Martin Anderson is one of those "in-and-outers" that G. Calvin MacKenzie speaks of. He performed similar functions in domestic policy research and development for presidents Nixon and Reagan. Definitely not a hanger-on, Anderson participates, then returns to the Hoover Institution to observe. He left in the second year of the Reagan administration, which certainly doesn't disqualify him from writing about it but does suggest the need for verification when it comes to his analysis of events that occurred after his departure, like the Iran-contra imbroglio.

Anderson's book is well structured. He begins with an analysis of worldwide political and economic change. He then turns to Reagan as a candidate, with a particularly fascinating account of his speechmaking style. This is followed in turn by examples of Reagan's vision, the birth and growth of Reaganomics, the operation of the Reagan White House, and the Iran-contra affair. In each case, Anderson's method is to provide the background for operations and decisions. Thus he supplies both the rationale for the Reagan approach and a sense of continuity that others find lacking.

For political scientists the chapters on the Reagan White House are especially valuable. Anderson identifies the president's management techniques: to establish strategic priorities, adapt tactical priorities to the situation, delegate authority, and negotiate (pp. 238–84). This mix of substantive policy direction and political flexibility bedeviled his opponents while earning him the designation of the "teflon president."

Anderson also offers interesting comparisons between Nixon and Reagan, revealing the strengths and weaknesses of each. Nixon was the "boss"; Reagan was the "governor."

> These two words sum up the dramatically different styles the two men used to manage and administer. Nixon used a no-nonsense, authoritarian style, barking out commands right and left. . . . He rarely felt comfortable with people. . . . Nixon's instinct is to plan and plot. . . .
>
> Reagan was just the opposite. . . . He liked people, liked all of them, a lot. He liked to meet them, talk to them, hear their problems. He rarely planned and never plotted. And he does not bark commands (pp. 278–9).

Anderson's profile of Reagan complements others in this set of books. Taken together they portray a president devoted to change but possessing a nonthreatening style. Thus, he prospered politically, as his adversaries consistently underestimated him. Seemingly, Reagan liked it that way. And why not? Winning is easy when expectations are low.

William A. Niskanen's account of *Reaganomics* should be on the short shelf of any dedicated student of the Reagan era. Compared to Anderson, he is much more the economist in his analysis. And his book is less a memoir than a critical review of economic policy. That does not make the book more interesting for political scientists—only very useful to them.

Niskanen provides a descriptive review of the intellectual roots of Reagan's economic plan, then a detailed summary and critique of its major components. Many accounts of Reaganomics treat only the 1981 tax and budget cuts. Niskanen includes these subjects, of course, but he also considers other important issues with significant impact on the economy, for instance, regulation, antitrust policy, trade, foreign debt, and the status of financial institutions. He also identifies the winners and losers of the Reagan policies. He has produced a virtual handbook of economic policy during the Reagan years, a fine reference source for those simply wanting to know the scope and sequence of actions taken.

One chapter in particular marks the author as an economist in residence. For whatever reason, Niskanen decided to include a discussion of "people and processes." As for the people, he is quite uncomfortable in doing what comes so naturally to the other authors, that is, commenting on the personalities with whom he worked: "I write these observations with some reservations. . . . If I have been unfair, . . . I apologize in advance" (p. 283). His analysis of the president squares with that of others: "Ronald Reagan has been the most transparent person in American public life. . . . He is at peace with himself, comfortable with his convictions, intuitive rather than analytical, youthful in spirit, infectiously optimistic, and gracious in his personal relations. The contrast with Richard Nixon could not be more dramatic" (p. 284). Most of his other profiles are brief and offer little that is new (or for which Niskanen has to be apologetic).

The discussion of processes is also brief but revealing. Most economists are impatient with political processes. No doubt Niskanen speaks for them in describing what happens when economic planning meets politics. "For most of those who worked on the substantive dimensions of economic policy, the White House political review was a 'black hole.' We saw what went into this process and what came out, but we had no understanding of the considerations and reasoning that led to the decisions made as a result" (pp. 306-7).

Niskanen credits Reagan with "the most serious attempt to change the course of U.S. economic policy since the New Deal" (p. 3). In the end, however, he concludes that "there was no Reagan revolution" (p. 332). The president failed to achieve substantial progress toward his announced economic goals, and he "left us with several substantial new problems" (p. 332). Here is an accounting not unlike Stockman's but without the self-serving arrogance.

One measure of the impact of a presidency is the number of books it spawns. There is little doubt that many more books will be written on the Reagan years by insiders and scholars. The total production will surpass even that of the Roosevelt years. It is unfortunate that commercialism has seemingly come to be the dominant factor in publishing these accounts, since it demands sensationalsim and a rush to print. That does not entirely vitiate the utility of these books for scholars but it certainly sets special conditions for their use. Verification is essential to realize the aforementioned advantages of memoirs.

Perhaps more substantial memoirs of the Reagan years are forthcoming. George Shultz and James Baker could tell us a lot. Baker will even have the advantage of comparative analysis between Reagan and Bush. No one of the six books reviewed here should serve as the model for subsequent contributions (bad approach with the first four, a limited perspective with the last two). Nor should Shultz or Baker turn to the books written by the insiders of the Carter and Nixon years (with the guarded exception of the Kissinger volumes).

I suggest they use as a model Dean Acheson's memoirs, *Present at the Creation.* Place it beside any one of the six books reviewed here and you will understand my recommendation. Here is a substantial work produced over a period of several years (Acheson ended his service as Secretary of State in 1953 and published his book in 1969). The skeptic in regard to any one reconstruction of events in the book has available references as starting points for verification. Acheson got help too, but from research assistants, not professional writers. The book is not objective. Acheson candidly tells his story from his perspective. But it was his intention to make a contribution to his craft and to the history of the times. These are decent goals, quite within the reach of many among today's public servants, I should think. They should be encouraged to try.

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Forthcoming in December

The following articles, research note, and controversies are tentatively scheduled for publication in the December 1989 issue.

David P. Baron and John Ferejohn. "Bargaining in Legislatures."

Clive Bean and Anthony Mughan. "Leadership Effects in Parliamentary Elections of Australia and Britain."

Paul Brace, Youssef Cohen, Virginia Gray, and David Lowery. "How Much Do Interest Groups Influence State Economic Growth?" A Controversy.

Stanley C. Brubaker and Ronald J. Terchek. "Punishing Liberals or Rehabilitating Liberalism?" A Controversy.

David Estlund, Jeremy Waldron, Bernard Grofman, and Scott Feld. "Democratic Theory and the Public Interest: Condorcet and Rousseau Revisited." A Controversy.

Michael Goldfield. "Worker Insurgency, Radical Organization, and New Deal Labor Legislation."

Joanne Gowa. "Bipolarity, Multipolarity, and Free Trade."

Nancy J. Hirschmann. "Freedom, Recognition, and Obligation: A Feminist Approach to Political Theory."

John E. Jackson and David C. King. "Public Goods, Private Interests, and Representation."

Michael D. McGinnis and John T. Williams. "Change and Stability in Superpower Rivalry."

Michael B. MacKuen, Robert S. Erikson, and James A. Stimson. "Macropartisanship."

Alex Mintz. "Guns versus Butter: A Disaggregated Analysis." A Research Note.

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POLITICAL THEORY

The Conquest of Politics: Liberal Philosophy in Democratic Times. By Benjamin Barber. Princeton: Princeton University Press, 1988. 220p. $25.00.

The Blue Guitar: Political Representation and Community. By Nancy L. Schwartz. Chicago: University of Chicago Press, 1988. 181p. $24.95.

These two books on democratic theory work well together. In his collection of revised essays, Benjamin Barber criticizes the reductionist and foundationalist tendencies of recent analytic political philosophy and suggests that political theory should encourage democratic conversation and informed political judgment about irreducibly political things. Nancy Schwartz extends Barber's argument in two ways. She provides a rich account of the social and theoretical resources democratic citizens can draw on to make sound political judgments, and she finds a home for this conception of the democratic citizen within the organizational structure of representative government.

While political theory may appear to have flourished over the last three decades; according to Barber, it has been actually conquered by well-meaning, mostly liberal, philosophers, who have been more concerned with logic and epistemology than with the democratic constitution of the common good. As a result, what we have now "is a distortion of our sense of the political that has undermined our politics at least a little and our philosophical understanding of politics a good deal more" (p. 5). How has this happened?

The first philosopher-conqueror in Barber's story is Bertrand Russell, whom Barber characterizes as an aristocratic liberal driven by an empiricist epistemology and metaphysics to dichotomize political possibilities: the anarchy of liberty and the dominion of power left no room for any other form of politics. Barber argues that personally, Russell was unable to accept this and hoped that justice could be had once we renounced our "love of power" (p. 51). But, Barber concludes, thinking about politics requires more than a call for the "love of justice," and it cannot be modeled on our thinking about nature, which is all Russell leaves us with in the end.

There is a compassion in Barber's criticism of Russell the philosophical skeptic and political solipsist that is absent from the two chapters on Rawls and Nozick. Perhaps it is because of Russell's own political engagement and the criticism he has suffered at the hands of philosophers who have dismissed his political writings as unphilosophical. However, Russell has probably not been as influential a political philosopher as Rawls or Nozick even in the unwanted way Barber describes. This misplaced emphasis shows up most clearly in Barber's depreciation of Rawls.

According to Barber, Rawls puts aside too many questions of political sociology and history for the sake of managing his philosophical derivation of principles of justice. Fair enough. He is a virtuoso when it comes to managing this philosophical argument, but as a political theorist he falls short. Nozick combines the search for philosophical foundations with an even more virulent reductionism to draw Barber's full fire. This is Barber at his best. He is too quick in characterizing Rawls's more recent political revisions of his theory as "meanderings," but in Nozick's case there have been no such revisions, no response to his critics. The bald assertions and depoliticized examples are still there, and Barber is right to treat them roughly.

In contrast, Ackerman, Oakeshott, MacIntyre, and Beiner have tried to avoid the lure of foundationalism in order to come to terms with politics in a language that joins theory with practice. While they have not succeeded, Barber hopes to make greater use of their failures to point us in the right direction. He takes advantage of his own considerable skill as a playwright to lampoon Ackerman's ideal of a constrained, neutral conversation. The more direct criticisms of MacIntyre and Beiner are familiar by now, but no less convincing:

neither provides us with a conception of political virtue or political judgment suited to the modern world of citizens acting together. The chapter on Oakeshott is by far the most valuable contribution in this volume. Barber is right again; it is very difficult to capsulize Oakeshott's arguments and still do justice to his prose. Barber's analysis of the categories and arguments of *On Human Conduct* leads to the sound conclusion that "if we ignore Oakeshott's definitions of theory and attend to his own theorizing, we will discover a useful alternative to the foundationalist tendencies of modern liberalism" (p. 175).

In short, Barber is claiming that political judgment is judgment made among citizens as they try to come to terms with political conflicts and political aspirations in a process of "reflective disequilibrium." Political theory, he believes, should enter this conversation committed to democratic norms of participation and equality. In *The Blue Guitar* Nancy Schwartz tells us more about how citizens can carry on such a conversation and, by her own example, how political theory can aid them. Her thesis is that representative political institutions can foster both community and autonomy.

In order to clear the way for this theory of "constitutive representation," Schwartz subsumes the conventional delegate and trustee concepts of representation under a single "transmission belt theory," in which representatives respond more or less faithfully to the wants and interests of constituents, transmitting these wants and interests to the government, which then must keep the peace. When this happens, constituents are relatively satisfied and states are sovereign. But representative government, according to Schwartz, can be and sometimes is much more than this. It can enable individuals to reflect critically on their wants and interests and sometimes change them. It can enable individuals to see themselves as belonging to a community and possessing certain values, institutions, and practices in common. It can enable individuals to achieve a level of self-legislative autonomy not through renewable contracts but through an identity-shaping process of covenanting and shared deliberation. Representation does not have to be a handmaiden to state sovereignty.

This sounds suspiciously like another call for participatory democracy. While Schwartz does believe that citizens should indeed participate, the transformation of private individuals into public citizens can occur within single-member districts organized under first-past-the-post rules. It is this organizational structure that makes meaningful, constitutive political dialogue possible. At-large elections and proportional representation dilute political discourse by creating a more abstract political space and a more tenuous link between representatives and constituents.

Unfortunately, legislative districting and apportionment have been viewed too narrowly, argues Schwartz, in part as a result of the dominance of the judicial preoccupation with the standard of "One person, one vote." To begin to remedy this, Schwartz takes us back to the tradition. In Plato, Rousseau, and Marx she identifies a rudimentary concept of constitutive representation in the figure of the political founder. In Marx and Weber she finds a conception of local politics that is both fraternal and organized around the appropriation of common goods. But the most important classical source for Schwartz's theory is Hegel. "Political individuality" is the first virtue of democratic citizens. It is a Hegelian synthesis of personhood and membership that one acquires "by choosing how one's intermediate community shall be defined—who the relevant others are, and how they should chart the path. It is a process that cannot be done alone, if objective value comes from agreement with concrete others" (p. 102). And when citizens enjoy this kind of political individuality, their representatives can "depict things as they really are, in their plenitude" (p. 145).

Schwartz's theory of constitutive representation may still strike some skeptics as utopian. She does not draw on contemporary case studies, so much as Renaissance Florence, to defend her view. However, as the 1990 census and the 1992 elections get closer, "internal boundaries" are already achieving an unprecedented salience for political parties. This is a very fine book, which bridges past and future by casting representation in a new light. In Wallace Stevens's words, "Things as they are/ Are changed upon the blue guitar." But they remain recognizable, and in this case they are changed for the better.

STEPHEN L. ESQUITH

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Book Reviews: Political Theory

Hobbes's Political Theory. By Deborah Baumgold. Cambridge: Cambridge University Press, 1988. 211p. $39.50.

In this provocative and interesting book Deborah Baumgold attempts to show that Hobbes was primarily an analyst of elite political behavior and of institutional design rather than an "individualist political thinker" (p. 1), as most modern critics (until very recently, at least) have assumed. The "assumption that everyman . . . is the principal subject of his political analysis" (p. 2), she suggests, is the rock on which most interpreters have foundered. Thus, in her view, Hobbes was more interested in understanding and frustrating the political designs of certain ambitious elites than in the general problem of securing the obedience of ordinary subjects to their sovereign. The *central* concern of his political philosophy, on her account, is not the problem of political obligation, but the phenomenon of political ambition (p. 122). Hobbes's focus was on the art of government in an era when monarchs were attempting to consolidate their authority in the face of challenges from powerful subordinate groups.

Baumgold's strategy of argument is dominated by two moves. First, she focuses her attention on the "political chapters" (p. 12) of Hobbes's works. In *Leviathan* these include (at least) chapters 13–31. By concentrating on some of the more neglected among these chapters she is enabled to depict Hobbes as a constitutional theorist who practices "a social science built around analysis of the constitutive roles and functions of state and society" (p. 11). Second, Baumgold suggests that Hobbes's arguments about these constitutional topics are generally framed in response to a set of issues that arose in England in the 1630s and early 1640s (p. 3). By setting his work within the context of these (mainly prewar) constitutional debates, she attempts to refute the commonplace image of Hobbes as a theorist of arbitrary government. Hobbes's political absolutism is undeniable, but Baumgold argues that his principal interest was in the sound institutions and policies on which the well-being of ordinary subjects depended.

Baumgold's book is generally consistent with, and makes a significant contribution to, a new image of Hobbes that has begun to emerge during the past few years. Her focus on the institutional and policy arguments developed in part 2 of *Leviathan* provides a valuable antidote to the preoccupation with the problems of consent and obligation that have long dominated Hobbes scholarship; and her accounts of Hobbes's theory of authorization and of the obligations of soldiers are particularly interesting. On the whole her attempt to trace the connections between the debates of the 1630s and 1640s and Hobbes's arguments in *Leviathan* is successful.

But Baumgold's focus on these arguments is also the source of the principal weakness of her book, for it is only by neglecting the remainder of his arguments—which occupy the bulk of parts 1, 3, and 4 in *Leviathan*—that she is able to make her rather striking negative claims seem plausible. Thus her suggestions that Hobbes was essentially a constitutional thinker and *not* a practitioner of individualistic political reasoning, that he was concerned with the ambitious designs of elites and *not* with the conduct of ordinary subjects, and that his interests lay in the art of government and *not* in the problems of obligation and consent all are flawed by her failure adequately to confront the very considerable evidence to the contrary. As she concedes on the penultimate page of her book, Hobbes held "the folly of the common people" responsible for the civil war along with "the eloquence of ambitious men" (p. 134). Baumgold is absolutely right to allege that Hobbes's political theory has been distorted by previous interpreters' failure to look far beyond part 1 and certain selected chapters of part 2 of *Leviathan,* but her own highly selective approach to the text vitiates her attempts to characterize his political theory as a whole.

Despite these weaknesses in this book's main thesis, its contributions are genius; for, among other things, it calls to our attention the fact that for Hobbes the "relation between protection and obedience" was "mutual" from the point of view of sovereigns as well as that of subjects. It thus provides valuable evidence that Hobbes's designs as a political theorist were far more complex and ambitious than many of those who have read him solely as a contractarian thinker have hitherto realized.

DAVID JOHNSTON

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Philosophical Critiques of Policy Analysis: Lindblom, Habermas, and the Great Society. By Lance deHaven-Smith. Gainesville, FL: University of Florida Press, 1988. 156p. $16.00.

In this work, Lance deHaven-Smith attempts to place policy analysis in a broader theoretical framework. Despite its title, the book does not examine philosophical critiques of policy analysis. Instead, it articulates the assumptions behind the Great Society efforts to alleviate poverty, presents contemporary criticisms of that effort, and then suggests how the theories of Charles Lindblom and Jçurgen Habermas can contribute to a better understanding of the impact of Great Society programs. DeHaven-Smith's primary concern is the evaluation of policy, and he concludes that a multiple-theoretical approach to assessing government programs provides greater insight into their impact than a single explanatory model.

DeHaven-Smith does a good job of summarizing the hypotheses about poverty that motivated the Great Society. He finds that the culture-of-poverty idea is no longer seriously challenged but that explanations of its causes differ. The Johnson administration people believed that isolation from social and political institutions was the most important cause of poverty and originally worked to integrate the poor into the system. The Reagan people, in contrast, contended that government programs created a welfare dependency syndrome that perpetuated poverty. Drawing on evaluations of the Great Society, deHaven-Smith argues that it had no significant long-term effect on the general level of poverty, that it did improve the situation of the elderly and blacks, and that some programs had useful results. The real question for the author, however, is how more inclusive theoretical perspectives might affect interpretation of the Great Society.

Using the theories of Lindblom and Habermas, deHaven-Smith claims to uncover effects of Great Society programs that are not easily identified by policy analysis that concentrates on specific program impacts. In his view, Lindblom's approach indicates that broad politico-economic mechanisms manipulated by authority figures inculcate the limited horizons of the poor and constrict opportunities for them. DeHaven-Smith then uses Habermas' ideas to suggest that the Great Society lost popular legitimacy when it moved from attempting to eliminate poverty to providing greater welfare. When this happened, policy makers began to paper over conflicts in society and limit dialogue among classes. DeHaven-Smith concludes that the gains that blacks made under the Great Society are more easily discovered using the theoretical lens of Lindblom and that Habermas' perspective facilitates recognition that the Great Society created a new "grants economy" that subsidized many in addition to the poor.

Somewhat ironically, in view of his argument for a broader philosophical context for policy analysis, deHaven-Smith's conception of policy analysis itself is rather narrow. By concentrating on the evaluation phase of policy analysis, he is able to apply his multiple theory approach fairly easily, and usefully. The question how valuable and practical such an approach would be during the actual process of designing and implementing programs is, however, not directly addressed. Moreover, deHaven-Smith's discussion of the impact of the Great Society might have gained in depth if he had utilized the interpretations of Frances Fox Piven and Richard A. Cloward in *Regulating the Poor* (1971). Finally, deHaven-Smith rightly emphasizes the importance of the 1964 Civil Rights Act, but he is wrong when he states that it did not apply to sexual discrimination (p. 20) and that it included protections against discrimination in housing (p. 23).

This book has much to recommend it. Its insistence on a broader philosophical framework for policy analysis is a step in the right direction. The author's application of the ideas of Lindblom and Habermas is heuristic in the best sense of that term. The discussion of the Great Society is comprehensive yet succinct; and it is in discussions of that program, rather than those about policy analysis, that the book will probably find its greatest use.

ROBERT HEINEMAN

Alfred University

Book Reviews: Political Theory

The Political Ideas of Leo Strauss. By Shadia B. Drury. New York: St. Martin's, 1988. 256p. $29.95.

Shadia Drury has written a historicist criticism of the work of Leo Strauss. Her interest in Strauss and his circle was stimulated by Strauss's influence in the United States and his growing influence in Canada.

Drury's primary concern is with Strauss's theory of esoteric and exoteric writing rather than his substantive contribution to political philosophy. Strauss held that all true philosophers were inevitably in a state of tension with their society and that this required them to exercise a prudent reserve especially when presenting their ideas in writing. He attempted to show in his own writings that in reading masterworks it was necessary to cultivate the ability to read between the lines. For Strauss the essential problem lay in the distance between the two poles of convention and nature, nature understood in the Platonic-Aristotelian sense as providing a standard for judging society and the individual not only apart from their historical setting but in relation to the highest human type and the best regime.

If we understand Drury properly, under historicist auspices this dualism between nature and convention is no longer necessary. The standard now is the cumulative social sentiments of the time, which are designated as morality. Secrecy would be an affront to this morality, for it might suggest that there was something wrong with this morality and it would be presumptuous to sit in judgment on prevailing social sentiment or the various currents that contribute to its formation. Drury seems to favor an open society with a free exchange of ideas and cites J. S. Mill in support of her case. However, Drury neglects to mention that Mill not only fudged in his autobiography but took drastic steps to conceal his differences with the philosophical radicals arising from the influence of Coleridgean conservatism on Mill's views. His views on politics and society were much larger and far more complex than Drury suggests, and this is given additional weight in the light of his sympathy with Tocqueville's fear of an unjust majority in the emerging democracy of the United States. This suggests that unlike Drury, Mill did not believe that moral and intellectual truth was coterminous with prevailing opinion either among ordinary citizens or elites.

However, the center of interest in this book is the alleged secret doctrines of Strauss himself. Drury's account may be regarded as one of the more curious episodes in the history of Western political science. Differences that have arisen in recent years among some of Strauss's students prompted the author to conclude that Strauss wrote and taught with a double doctrine in mind. One doctrine was taught to those who had the potential to be philosophers and the second was addressed to a lesser breed who had the promise of developing into gentlemen statesmen or their teachers. According to Drury the philosophical group was introduced to an immoralist view of political life based upon an intermingling of elements from Plato, Machiavelli, and Nietzsche in the interest of manipulating and managing a society. The gentlemen were taught respectable versions of the natural right doctrine and moral obligation. Although some of these students had studied and worked with Strauss for long periods of time, they never realized that they had been singled out for special consideration. Drury means to convey that the reputation of Strauss as a natural right political philosopher with a high-minded approach to political life is simply false in all essentials. Drury concludes with the prediction that the rising conflict between the gentlemen and the immoralist philosophers will give greater currency to his esoteric doctrine and thereby put an end to his influence.

Drury's starting point for her charge of immoralism is the classical dictum that the intellectual virtues leading to science and wisdom are not dependent on moral virtue, which is composed of those habits of character that make for self-restraint and a sense of justice. To view this as a license for moral indifference is a major mistake in this book. The dictum simply points to the fact that a university would not appoint a mathematician, physicist, or any theoretical person primarily because of his or her moral character. In the field of practical activity the case is different, for here moral judgment is essential for practical judgments. As Jaeger expressed it, *phronesis* or practical reason "is merely the housekeeper and not the mistress of the soul." And yet the role of practical reason should not be underestimated. Although it derives its basic principles from theoretical reason, at the level of politics practical reason is decisive in ordering activities, including that of science. Drury compounds her mistake when she attributes a hostile attitude toward science to Strauss when in fact what he objects to are narrow conceptions of science and a scientism that unduly intrudes into the appropriate sphere of practical reason.

Unfortunately, Drury's citations and quotations are often misleading, tendentious, inaccurate, or taken out of context. Her most serious charge is that Strauss, following Plato, secretly advocated a tyranny of the wise, meaning a form of rule outside the law. Drury neglected to mention that tyranny includes ruling in one's own interest, nor does she refer to numerous passages in Plato and Strauss indicating the enormous difficulty in combining in one person or a class both philosophic and practical wisdom as well as the great reluctance of truly theoretical persons to engage in practical activity. There is no mention of the need for special historical and social circumstances, but above all Drury disregards Aristotle's total dismissal of the idea of a philosopher-king.

Other controversial points include a claim that Strauss identified the public good with collective selfishness. The reference is to a section in *Natural Right and History* in which Strauss is explaining the conventionalist attitude, which is certainly not his own. Drury holds that Strauss secretly uses Machiavelli and Nietzsche as mouthpieces. She has Strauss privately agreeing with Machiavelli that cities are founded on crime and terror. Strauss's effort to contradict Machiavelli by citing the founding of the United States as an example of a just founding is nullified by Drury on the ground that Strauss mentioned in the same place the Louisiana Purchase, referring to it as a crooked piece of business. Here the context is ignored, for Strauss writes that to use the presumed illegality of the purchase to cloud the justice of the founding would be Machiavellian mischievousness. Space will permit only one more example of improper citation. In his study of Machiavelli, Strauss observes that ancient writers taught Machiavelli's doctrines covertly. Drury not only substitutes the word philosophers for writers but chooses to ignore Strauss's reference, in the same place, to the famous interchange between Socrates and Thrasymachus in Plato's *Republic* concerning the unrighteous use of power. There are many such discrepancies in this book.

STANLEY T. GABIS

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Althusser: The Detour of Theory. By Gregory Elliott. New York: Verso, 1987. 359p. $39.95 cloth, $17.95 paper.

Louis Althusser was at the center of the international revival of Marxist theory in the 1960s and 1970s. His two best-known books, *For Marx* and the collaborative work *Reading Capital,* both published in 1965, had an enormous impact, perhaps for three principal reasons. First, Althusser's discovery of an "epistemological break" separating the "ideological" Marx of the *Paris Manuscripts* from the "scientific" Marx of *Capital*—a frontal challenge to the humanist variants of Marxism current on the European Left after 1956—generated bitter controversy. Second, the attempt by Althusser and collaborators such as Etienne Balibar to provide a rigorous conceptual reconstruction of the main theoretical principles of historical materialism offered many young radical scholars an apparently useful framework within which to conduct research. Third, Althusser's proclaimed "return to Marx"—his attempt to remove accumulated misreadings and misunderstandings separating us from the real and hidden meaning of Marx's texts—struck a chord among a generation of Western intellectuals radicalized by 1968 and its aftermath. But if, as this suggests, politics contributed to Althusser's rise, it also played a decisive role in his fall. The "crisis of Marxism" of the late 1970s, reflecting the decline of the European Communist parties and the disillusionment of the generation of 1968, was at its most acute in France, where the *nouveaux philosophes* were able effectively to reduce all versions of Marxism to apologias for the Gulag. Althusser, silenced by personal tragedy, also suffered intellectual eclipse. Soon Luc Ferry and Alain Renaut could write, "Althusserianism . . . seems very dated and irresistibly evokes, like the Beatles' music or Godard's first films, a recent but vanished past" (*La Pensée* 68 [1985]: 200).

Gregory Elliott's study is intended in part to rescue Althusser from such dismissals—not that it is in any sense uncritical of its subject. On the contrary, Elliott remorselessly explores the weaknesses of Althusser's thought. He is, however, committed to taking Althusser—whom he places "among the most significant Marxist thinkers of the century"—seriously and sums up his own position as "anti-anti-Althusserian" (p. 10). Like Sartre a Marxist faute de mieux, Elliott rejects what he regards as Althusser's dogmatism but treats him with respect. The result is undoubtedly the best, most comprehensive study of Althusser's thought that we have. This is not, however, because Elliott comes up with any particularly startling new insight into Althusser's philosophical evolution. On the contrary, he largely follows the path laid by a number of earlier critiques—Norman Geras's in *Literature of Revolution* (1986), Edward Thompson's *Poverty of Theory* (1978), Perry Anderson's *Arguments within English Marxism* (1980), Ted Benton's *Rise and Fall of Structural Marxism* (1984), and my *Althusser's Marxism* (1976), and *Is There a Future for Marxism?* (1982), works whose influence he scrupulously acknowledges.

The real originality of Elliott's study lies in its employment of the skills of historical scholarship to explore Althusser's thought. Elliott reveals the existence of voluminous unpublished writings by Althusser, to which he was able to gain access and which cast new light on the published work. We are offered, for example, a tantalizing glimpse of the larger manuscript *De la superstructure,* from which Althusser's famous essay "Ideology and the Ideological State Apparatuses" (1970) was drawn. But Elliott's substantial achievement lies in his use of both such written material and extensive interviews with Althusser's circle to reconstruct the political context of their theoretical work. Far more successfully than any previous study, Elliott is able to trace the interaction between Althusser's philosophical evolution (e.g., the rejection in 1967–68 of the "theoreticism" of his earlier work and eventual redefinition of philosophy as "the class struggle in theory") and developments within the French Communist party. Finally putting to rest the canard that Althusser's "antihumanism" was an attempt to rehabilitate Stalinism, Elliott shows that Maoism provided the leitmotif of his development, which underlaying, for example, the stress laid on production relations rather than productive forces in *Reading Capital,* and pushed Althusser towards a more strident "Marxism-Leninism" as the Chinese Cultural Revolution and 1968 together radicalized Mao's Western followers. The collapse of European Maoism in the late 1970s helped to provoke Althusser's own personal version of the crisis of Marxism: Elliott uses little-known texts to show how far Althusser moved in this period away from the idea that the mere "return to Marx" was sufficient to resolve the perplexities of the Western Left. If *Althusser: The Detour of Theory* succeeds in helping to revive serious discussion of its subject, it will not be the last word on Althusser's theories. But it is hard to see how anyone could improve on Elliott's exploration of the changing historical context of their formulation and revision.

ALEX CALLINICOS

University of York

Liberalism, Politics, Ideology and the Market. By John A. Hall, Chapel Hill: University of North Carolina Press, 1988. 253p. $24.95 cloth; $9.95 paper.

This is an attempt to vindicate liberalism against historians and political theorists like Anthony Arblaster, who argue that it has become obsessively conservative in its defense of private property rights and willing to pay virtually any price—including authoritarianism—to maintain existing inequalities. For Hall, instead, liberalism is chiefly characterized by its belief in the equal moral worth of all individuals, its adherence to an "open society" based on negative liberty, and its skepticism of "totalizing ideologies." Its relationship to capitalism he portrays as dominated by an admirable fear of political tyranny rather than a desire to increase or perpetuate unjust inequality. These values, he insists, need to be shored up against attacks from both the Left and Right, in order to defend the more sophisticated view that "capitalism can help but may hinder liberal aims" (p. 36).

The book is divided into three sections: an overview of the eighteenth- and early-nineteenth-century origins of liberalism; some consideration of its supposed nineteenth- and twentieth-century failures; and an attempt to establish a general theory of contemporary liberalism on the basis of developments since 1945. The first section surveys the development of a liberal epistemology wherein all knowledge is derived from the senses. Hall argues that the empiricism of Hume, Bentham, and others was not inevitably conservative or positivistically incapable of envisioning social improvement but rather was essentially democratic in its implications, since all individuals were conceived to have the same passions and capability for happiness, as well as, in Kant's terms, equal moral worth. The scientific outlook that grew from empiricist premises is for Hall peculiarly compatible with liberalism; in both, particularly, freedom of opinion is central. Nonetheless the acceptance of this premise left open the possibility, explored by Hall in relation to J. S. Mill, that the appeal to freedom of opinion left no absolute values, no superior vision of the good society, merely ethical relativism. The second component of classical liberalism for Hall was a view of commerce according to which (for Hume, Smith and others) the bases of political liberty were seen to lie in economic freedom, whose ordering of self-interest also provided a basis for social stability and the rule of law. The parallelism between the market and democracy, with both supposedly maintaining the sovereignty of individual choice when unconstrained, was essential to the nineteenth-century success of this vision.

Hall's second section concentrates upon the late-nineteenth and early-twentieth-century loss of confidence in liberal ideals. Challenges to the latter came from Nietzschian irrationalism and the claims to higher, as well as darker, forms of authenticity and from the doubt that any "real self" existed that could serve as the basis of moral behavior, of which existentialism is the best-known modern form. These objections are fended off by a defense of Goffman's notion of role playing and the use of "masks," by which both individualism and the need for special interaction are reconciled, while a "civility" (p. 87) is maintained that is essential for political life and whose cultivation is superior to the search for authenticity. Liberalism was also challenged by the rise of the workers' movement and the notion that the state was held prisoner by capitalists. Hall here concentrates on the relationships between state and workers in Britain and Germany, arguing that while capitalists did ally themselves with authoritarian regimes, the "logic" (p. 119) of scientific and industrial development favors more democratic societies. He also treats the question of the inevitability of war as the outcome of capitalistic imperialism, contending instead that it is the result of special interests' predominating—historically of political authoritarianism in particular—rather than of liberalism per se.

Liberalism after 1945 has lacked the confidence and cohesiveness of its more ebullient nineteenth-century ancestors, though it is now stronger than at any previous point in the twentieth-century. Hall applauds the creation of compromise forms of social liberalism but is principally concerned to reconstruct the moral basis of modern liberalism, basing this on a skeptical philosophy of science, a utilitarianism organized around the notion of "settling for less" than complete sociability or authentic identity, and a theory of liberty that includes the provision of a basic subsistence, hostility to arbitrariness, and popular political participation. He concludes by commending industrialization, even by forced means, as a path to liberal ends, as well as "successful nationalism" (p. 204). Liberalism does not, Hall contends, require a fixation on the rule of the market; for a strong state is required both to ensure high levels of social skills and the preservation of rights. This is a thoughtful and interesting book that offers a compelling defense of social democracy. While its relevance probably could have been sharpened by further consideration of the conservative critique of liberalism—today much in the ascendancy by comparison with Marxism—this does not markedly affect its central contentions, which are defended with a laudable clarity and will doubtless prove of interest to a wide readership.

GREGORY CLAEYS

Washington University, St. Louis

The Politics of Being Mortal. By Alfred G. Killilea. Lexington, KY: University Press of Kentucky, 1988. 172p. $21.00.

There is no question that the author's theme is an interesting one, and one treated cursorily, if at all, in the literature of political science. It holds that death is natural and necessary and that the recognition of that fact yields potentially enormous individual and social benefits.

Alfred Killilea argues that as individuals face up to, and accept, their own mortality, they will recognize the value of limits and of human relationships; that death will give us all a sense of proportion and priorities. He asserts that after a century's avoidance of the subject because it was considered anathema to progress and success, our culture (U.S., Western, or capitalist generally, it is never quite clear) is now witnessing momentous attitude changes towards death, evidenced by the hospice movement, numerous popular works on death and dying, and more frankness in the discussion of organ donation and medical problems.

Killilea believes that these trends will lead to key insights, namely that "only in confronting human mortality can we appreciate the limitedness and the preciousness of life. . . . Nothing confirms our identity with other mortals and our mutual dependence as powerfully as death does. . . . Acknowledging our common mortality seems to be a catalyst to taking equality and community more seriously" (pp. 8–9); that "life makes sense only as it is shared with others" (p. 55); and that "the most precious achievement we can want is to give and extend life in others" (p. 100).

Killilea predicts, and clearly hopes, that such alterations in individual consciousness will have profound social, economic, and political ramifications, including a rejection of excessive competition, acquisition, and unbridled materialism; a reassertion of the values of equality and community; increased realization of participatory democracy; a mass rejection of nuclear deterrence strategy; the elimination of nuclear weapons; and solidarity with other peoples and cultures.

The Politics of Being Mortal can be characterized as a thoughtful, erudite, scholarly, readable essay suitable for seminars, tutorials, and collegial discussions. It is neither Marxist nor only for the religious. Its major value lies in its provocative theme, condensation of a wealth of bibliographic material, and captivating critiques. Killiliea reviews the works of many. He castigates the likes of Jacques Choron and Ernest Becker for arguing that death is intolerable, John Locke for encouraging acquisitiveness based on what Killilea calls a "sinister view of nature" and Michael Novak for laying the problems of capitalism at the feet of culture. He praises some, including Daniel Bell and Christopher Lasch (although claiming they do not go far enough in their discussions of the relationship between death and culture) and Jonathon Schell for his bold critique of deterrence strategy (and blames his later backsliding).

The weaknesses of the book include faults of which it often accuses others. There is the occasional lack of a sense of history and proportion—for example, in the suggestion that hedonism and greed have no history before, nor does altruism have a future after, the birth of capitalism (see p. 91). There are also ambiguities about causes and effects, such as whether the denial of death causes a denial of limits and the threat of nuclear arms or vice versa (see pp. 10, 98). Most troublesome are two assumptions. The first is that everyone gains the same perceptions, lessons, and insights from facing his or her own mortality. It seems more easily argued that the range of attitudinal and behavioral responses to such existential realities is quite broad and is not confined to an enhanced sense of brotherhood and responsibility to others. The second arguable assumption is that alterations in individual consciousness will cause transformations in U.S. social consciousness and then world consciousness and behavior in such a way as to make nuclear disarmament possible in the near future. Such leaps from individual perception to international behavior are tenuous at best.

In spite of such weaknesses, Killilea's book is worth a read for the material it summarizes and the degree to which it provokes thought. In his own words (with which I agree), "If it is the function of political theory to perceive new political possibilities and to explore and articulate alternatives to present restrictions upon the human need for creativity and community, then there is an urgent need to take stock of the significant political ramifications of people's attitudes toward death" (p. 124).

CARI SHAY

Western Oregon State College

Needs, Rights, and the Market. By David Levine. Boulder, CO: Lynne Rienner, 1988. 158p. $25.00.

An essay by an economist on the way needs and rights enter economic analysis promises that kind of philosophical economics that Amartya Sen has shown to be so fruitful, a fruitfulness appreciated by philosophers and largely ignored by economists. Levine lodges "needs" in concepts of personal autonomy and personality development and uses this interpretation to reveal how private enterprise fails to meet most of the needs of the people it is supposed to satisfy. These needs are expressed in various "modes of life," marked by a variety of individual consumption patterns. A person's needs are served by property, including human capital, but the extension of property to include the means of production infringes the need-serving property rights of those who do not own that kind of capital. Moreover, the system of private enterprise tends to develop spurious "needs" that it cannot fulfill. The definition of personal worth in money terms not only creates people of unequal worth, thus violating their rights to "equal regard," but also tends to lead capitalists themselves into postures of self-aggrandizement, thus undermining their own personalities. In this respect rentiers, who fail to use capital in ways integral to their own development, are especially blameworthy. Levine's chapters on wealth, capital, the market, and private enterprise develop these themes in a variety of insightful and clarifying ways.

In spite of the graceful and accessible exposition of these seminal topics, the analysis fails several tests. The crucial distinction between wants and needs is never clarified, leaving his treatment vulnerable to familiar arguments about utility monsters. Conflicting needs must be reconciled, but Levine offers no suggestions for how this is to be done. Further, unlike Rawls, other modern critics of utilitarianism (e.g., Rescher), and Okun, Levine fails to deal with the question of the effects of his proposals on productivity—how society is to produce the goods to serve the needs he outlines. Indeed, one gets the impression that Levine prefers equality in poverty to inequality in wealth, but the case is not argued.

Assumptions about the effects of market processes on personality are unsupported by evidence. For example, if the "self-aggrandizement" attributed to entrepreneurs means pursuit of reputation, would that pursuit be more avid for entrepreneurs than for scholars? If it means pursuit of power, are entrepreneurs more eager for power than bureaucrats or politicians? Economic motivations have been studied by a variety of industrial sociologists and psychologists, but Levine reveals no interest in these studies. Levine accepts the familiar claim that market-induced affluence expands needs for material goods more than other, nonmaterial, goods, a claim set forth without reference to such conflicting evidence as, for example, that presented by Ronald Inglehart in *The Silent Revolution* (1972) or the Michigan quality-of-life studies, both revealing a shift away from material interests in the more affluent market societies. In short, Levine is content to assume the properties of human behavior in various social contexts without consulting the disciplines that deal with these problems. Radical in a superficial sense, Levine's treatment fails to meet the most radical of all propositions: a fearless examination of the facts as these are slowly developed and revised by research in all the various disciplines of human knowledge.

ROBERT E. LANE

Yale University

Reflections on Political Identity. By Anne Norton. Baltimore: Johns Hopkins University Press, 1988. 209p. $24.50.

While political science has maintained a disciplined inattention to structures of thought that politicize the concept of desire, there are those—most notably the literary—who have displaced desire from its place as an asocial cause of valuing and behavior to the domain of symbolic interaction by drawing on thinkers who locate desire within a symbolic political economy. This conceptual move has enabled another move more familiar to those interested in the political significance of forms of apprehension, a move from an analysis of the valuing and desiring economies of persons to an analysis of the modes of representation through which collective relations are ordered. These moves are at the center of Anne Norton's analysis in *Reflections on Political*

Identity. Drawing heavily on the writings of Jacques Lacan, she maps a view of the emerging self that demonstrates the radical entanglement between one's psyche and one's orientation to the political domain, arguing, after Lacan, that inasmuch as desire is mediated through a person's symbolic—and thus collective—relations, it can be shown to be always already political. This is not news to those in philosophical and literary disciplines (among others) whose versions of social theory have been influenced by recent Continental thought. But while the thinking of Lacan and its implications for a politics of desiring and identity is familiar to academicians in some disciplines, the importance of Norton's treatment is that she creates a bridge between the view of the self and identity emerging from Lacan's approach and more traditional modes for raising political questions.

Once the Lacanian approach to the self is introduced, Rousseau becomes Norton's vehicle for thinking through the relationships of the self to the order. Then the perspective of Victor Turner is invoked to link individual and collective identity politics to a ground plan, to systems of boundaries understood through the historical production of liminars (persons with socially marginal identities) and their activities. These conceptual vehicles, along with a healthy dose of Weberian thinking on issues of authority, carry the narrative through the familiar political issues of representation and violence. But these discussions are deflected from the typical discourses on such issues by a welter of insights from diverse cultural experiences—examples are drawn from such diverse groups as the Libyan military and the Japanese Samurai class—and by the ebb and flow of a broad range of theoretical vocabularies.

While much of the book may seem to be more of a series of jump-and-cuts than a unified narrative, a persistent conceptual theme organizes the entire discussion. Beginning with a strong thesis on the genesis and nature of meaning—as an interplay of identity and difference and presence and absence—Norton is able to demonstrate the analytic payoffs of this view in domains as diverse as the production of individual identity, the legitimation strategies of political leaders, and the motivations for violence embedded in various forms of the conceptual production of sameness and otherness.

Many will question some of Norton's theoretical representatives. For example, one can question whether the Rousseau she constructs has survived the deconstructive readings (which she herself reviews in the book) well enough to serve as a vehicle for thinking through the identity questions she raises. And some absences may be regarded as too glaring. For example, one can wonder why we are not introduced to René Girard, whose thinking on the relationship between identity and violence could both challenge and enhance her discussion. In addition, some may want a more robust rendering of the domain of "the political," which Norton simply allows to exist as a function of mediation, of the recognition of the social that is immanent in the more individual enactments of various forms of desiring and self-recognition.

But *Reflections on Political Identity* is not a traditional piece of scholarship. Its style is terse and at times even aphoristic. Rather than explicative and systematic, Norton's text is a mobilizing structure. It sets in motion a loose arrangement of diverse disciplinary discourses brought into confrontations that help the reader rethink (recode) traditional political problematics. The book, as the title implies, is thus a set of reflections; and the theoretical discourses that meet in these reflections should help political scientists to rethink the discursive economies of that heterogeneous set of statements that in a variety of ways becomes assembled under the rubric of "political science."

MICHAEL SHAPIRO

University of Hawaii, Manoa

The Spirit of Modern Republicanism: The Moral Vision of the American Founders and the Philosophy of John Locke. By Thomas L. Pangle. Chicago: University of Chicago Press, 1988. 334p. $22.50.

The Spirit of Modern Republicanism is an important, if idiosyncratic, interpretation of the modern liberal tradition as exemplified in the thought of John Locke and the U.S. founders. The scholarship is impressive. There are few major writings on Locke or the U.S. founders that are not cited and criticized, often trenchantly; and the original sources are equally well-mined. As much as one can quarrel with particular interpretations, Pangle's expertise in offering them cannot be disputed.

For Pangle, the seventeenth- and eighteenth-century liberals never climb to the heights of the ancients, who strove for a "higher, more solid form of human excellence" (p. 124) found only in the life of contemplation leading to the fullest possible self-knowledge. Yet, if not ascending to the grand intellectual virtues of the classical vision, the founders (Lockeans in action) are at least exemplars of what could be called the petty virtues of the active political life. Moderation, sociability, industriousness, and lawfulness are included in this prosaic, but not unimpressive, catalogue.

No short summary can do justice to the richness of Pangle's analysis, but if this theme sounds familiar in broad outline, it should. Pangle is playing a variation on a theme from Leo Strauss—and a slight one at that. Strauss is cited some 40 times in the text, never critically. Readers of Strauss will be familiar with many of Pangle's motifs both in substance and method, especially in his discussion of Locke.

Note, for example, the claim that the commentator unsullied by Nietzsche, Heidegger, or Marx is in a privileged position to "understand the author as [the author] understands himself"; the discovery that the author (in this case, Locke) admits his true obscurantist intentions and does not mean much or most of what he actually says (pp. 137–138); and the interpretation, which inevitably, emphasizes the unbridgeable gap between "ancients and moderns" and the ethical shortfalls of the latter.

Pangle's close attention to texts has its rewards, as is particularly apparent in the discussion of the founders. He argues convincingly the once-conventional view that the founders were liberals building a polity to protect life, liberty, and property and relying largely on enlightened self-interest to keep it stable. His case against the still-in-vogue "republican revisionist" readings of the founding is vitiated only by his urge to define the classical tradition as the transcendence of politics rather than as a particular form of politics. After all, antiquity includes Pericles as well as Socrates. Pangle's focusing attention on the meaning of classical republicanism is salutary nonetheless, as the revisionist literature, excluding John Pocock, tends toward imprecision on this score. However, Pangle muddies the waters himself in his brief concluding section. He refers to the continued "empirical foundation" of classical republicanism in U.S. statesmanship without giving any clear sense of what this means or how Lockeanism and republicanism are—or can be—conjoined (p. 277).

The imposition of the Straussian grid does its most serious harm in Pangle's treatment of Locke. Against the best Locke scholars of the day, including Peter Laslett, John Dunn, and James Tully, Pangle tends to view the role of religion in Locke's politics as purely instrumental. Locke "employ[s] biblical language" (p. 150) or "switch[es] to quasi-biblical or pious language" (p. 234) either to offer an "opiate" to poorly behaved men (contra Marx: not *just* to the masses) (p. 205), or to save his own hide.

Pangle is intent on making this charge stick to better draw the contrast between the "nobility" of the classical vision of the good life and the moral paucity of modern politics. The liberal republic is continuously described as a mere modus vivendi, "a mere means to safety and material well-being" (p. 117), among competing rational agents. The family is described in similar terms.

Such a view simply ignores Locke's understanding of natural law and the myriad obligations it imposes. As James Tully has shown, the obligations to preserve myself and to preserve others when doing so does not cause my own death, the obligations of parents toward children, and even the obligation not to let natural resources spoil are grounded in a deeply theological view. They rest on the fundamental dependence of humankind on God, being a product of God's workmanship.

So much of Locke's moral and political philosophy resides in this workmanship model that it is implausible to claim that he did not take it seriously. It is also apparent that Locke's liberalism is far from a modus vivendi. In Locke's own words, the law of nature "obliges every one" and reason "teaches all mankind . . . that being all equal and independent, no one ought to harm another in his life, health, liberty, or possessions. For men being all the workmanship of one omnipotent and infinitely wise maker, . . . there cannot be supposed any *subordination* among us, that may authorize us to destroy one another, as if we were made for one another's uses" (*Second Treatise*, 6).

Now, any number of objections can be raised against this vision. But it is a more profoundly moral one (as witnessed in its evocation of a Kantian kingdom of ends) than Pangle allows. And it is not without a nobility of its own.

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Marxism and Social Democracy: The Revisionist Debate 1896–1898. Edited and translated by H. Tudor and J. M. Tudor. Cambridge: Cambridge University Press, 1988. 384p. $49.50.

Marx: A Radical Critique. By Alan B. Carter. Boulder: Westview, 1988. 301p. $38.50.

It is commonplace that advanced capitalist societies have developed in ways that Marx did not foresee. The most thoughtful among his followers have understood that without revision Marxism would produce unintended consequences or simply become increasingly irrelevant. It may be that Marx was unable to foresee key developments because of his midnineteenth century perspective. This would call for supplements and revisions. Or, more seriously, systematic gaps in Marx's theoretical approach may have caused him to misidentify developments that would later undermine the agendas of the socialist Left. The essays, letters, and speeches collected by the Tudors explore the first possibility as it emerged in the 1890s within the German Social Democratic Workers party (SPD). Alan Carter explores the second possibility, arguing that the Left must reject Marxism if Marx's own radically democratic ideals are to remain on their agenda.

The SPD debates were sparked by Eduard Bernstein, who challenged the orthodox view within the SPD that a crisis of capitalism was objectively necessary and its revolutionary overthrow imminent. In Bernstein's view, the unexpected resilience of capitalism, the extension of the franchise, and the increasing successes of the SPD and other socialist parties in winning improvements for workers meant that it was both possible and necessary to transform society through democratic and evolutionary means. Karl Kautsky, August Bebel, Belfort Bax, Rosa Luxemburg, and others defended versions of the orthodox position.

The breadth and continuing relevance of the issues introduced by Bernstein are striking: he argued that the increased stability of capitalism in the late 1800s brought into question Marx's theories of class polarization and economic crisis. The development of welfare policies under Bismarck introduced questions about a new role of the state in defusing class conflict. An increasingly complex division of labor meant new difficulties for sudden, revolutionary transition as well as for planning in a socialist society. An increasing heterogeneity of labor suggested new difficulties with Marx's theories of class consciousness, which in turn raised questions about the nature of consciousness and political education. Indeed, the seeming irreducibility of consciousness suggested to Bernstein the need for a socialist ethics and theory of moral development. Finally, colonialism raised questions about how to assess global capitalist development and German nationalism. Bernstein saw capitalism as a vehicle for the progressive ethics of European civilization; various socialists rejected either this equation of moral progress and capitalism or Bernstein's claim that European ethics were necessarily superior to others, or both.

These essays and letters are remarkably readable. The Tudors have done an excellent job of translation and documentation; their selection and arrangement provides a sense for the breadth, seriousness, and continuity of the debates. The collection challenges our post-Leninist views of Marxist parties as rigidly doctrinaire: most party officials seem to have regarded the debates as encouragement for education, critical thinking, and political responsibility. John Stuart Mill would have been gratified. At the same time, these are not first-rate theoretical documents. They are remarkable more for the issues they raise—likely future difficulties for Marxism—than for the revisions they propose. Bernstein raised troubling questions, but his resolutions were not convincing. Looking back we can see that he was overly complacent about the stability of markets and failed to anticipate the role of the state in achieving the stabilizations that have in fact occurred. Bernstein understood less than his orthodox opponents about relations between global expansion and war. And his faith in progressive democratization left him unable to anticipate the rise of fascism.

Alan Carter takes his point of departure not only from the failures of socialist movements in advanced capitalist societies but also from the failure of Leninist movements and socialist states to achieve radical democracy. These failures, he argues, follow from defects intrinsic to Marx's theories: Marx's economism left his followers unable to appreciate either the power of the state and monopolies to stabilize capitalism, the determining role of politics and ideology, or the rise of a middle class of planners and technocrats. Marx's reliance on Hegel's logic produced a misplaced faith in the necessity of crisis and the emergence of democratic socialism. Not attentive to these difficulties with Marx, socialist states have unwittingly reproduced new kinds of class domination. Because, in Carter's view, all Marxists replicate Marx's reductions in one way or another, those with radically democratic commitments need to reject Marxism altogether. Carter seeks to go beyond Marx by first, replacing his base-superstructure model with one that shows the determinate role of politics, ideology, and the state in stabilizing and reproducing capitalism; second, replacing a key element of Marx's economism—the labor theory of value—with a theory sensitive to the constitution of value through demand; and third, showing how the technology that develops under capitalism can reproduce the domination of technocrats under socialism.

Carter's development of these arguments, however, is uneven. He too often substitutes rhetorical questions for analysis. He treats the weakest arguments in Marx (e.g., the generalities of *The Communist Manifesto)* as if they were the strongest. He presents his points as if they were unique, suggesting an innocence of debates among Marxists on these very issues—debates that began, as the Tudor's collection shows, almost a century ago. These debates deepened in the late 1920s and beyond: consider Gramsci and the Frankfurt School on culture, consciousness, the state, and technological rationality. Or consider Sraffa on Marx's economics or John Roemer's elegant reformulations, to which Carter refers but which he fails to digest. Carter's analysis is further burdened with a pre-Kantian empiricism (the view that all knowledge is either empirical or analytic), leading him to misunderstand the claims of Hegelian Marxists, critical theorists, and, indeed, Marx himself. Carter's attempt to trace every flaw in Marx back to Hegel is especially unconvincing. Because he reduces Hegel to his idealistic logic, he is unable to appreciate either Marx's reliance on Hegel's phenomenology or Marx's own critique of Hegel. Even those Marxists Carter does consider and for whom he has some sympathy—Poulantzas, Gerald Cohen, and Roemer, for example—he reads selectively, pushing them into mechanically determinist molds that fail to differentiate unique modes of explanation, such as functionalism in Cohen and structuralism in Poulantzas. Still, if one has the patience to separate the wheat from the chaff Carter makes some key points, especially those having to do with the consequences of technology and planning for class stratification, power, and democracy.

MARK WARREN

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Kantian Ethics and Socialism. By Harry van der Linden. Indianapolis: Hackett, 1988. 370p. $32.00 cloth, $10.00 paper.

Kantian Ethics and Socialism grows out of a long tradition that, since the days of Hermann Cohen and earlier, has sought to develop the socialist implications of Kantian ethics. It is nevertheless a welcome addition to the literature on Kant's practical thought—and not only because that tradition is relatively unfamiliar to English-speaking readers.

Van der Linden does not claim that Kant was himself a socialist but rather that socialism is the most consistent political and economic expression of the fundamental Kantian moral duty to promote the highest good possible in the world along with the related duty to treat humanity as an end in itself.

Van der Linden's argument combines clear and painstaking exegesis with rigorous reconstructive analysis. His purpose is not so much to state what Kant said as to show how a basically Kantian framework can provide the foundation for an adequate socialist ethics, thus saving the socialist tradition from the pitfalls of an amoral reliance on historical necessity.

Generally, Van der Linden treats the moral commitment to change society as central to Kant's practical thought (in contradistinction to treatments that make individual private duty and the struggle against inclination central). The author's reliance here on an extraneous definition of private (i.e., as "conservative," "unhistorical," and unconcerned with ultimate ends [p. 14–17]) may seem question-begging to some. Nevertheless, Van der Linden persuasively argues that active, public promotion of the highest good is an integral element of moral duty as Kant conceived it. The author's activist strategy is further served by an ingenious reading of the antinomy of practical reason, a reading meant to discredit the so-called moral postulate of God's existence and thus to discount Kant's avowed pessimism concerning the power of man (alone) to realize the highest good.

The implications of the author's minimalist reading of the highest good (as something like the promotion of peace and civil government) clearly emerge from his thoughtful and comprehensive discussion of history as a credible ground of moral hope. Van der Linden is at pains to support such a hope, while also wishing to discredit Kant's claim that nature guarantees legal progress (i.e., that "a nation of intelligent devils" can solve the problem of politics), inasmuch as insistence on such guarantees tends to undermine the significance and scope of genuine moral praxis. (It struck this reviewer that the author's challenge to Kant's argument, which he rejects on the basis of a somewhat tendentious appeal to Rawls's "original position," would be better served by a direct appeal to the facts of political life.)

What Van der Linden ultimately argues for is a serious expansion of the role of Kantian moral education, along with its radical extension into the economic and institutional spheres; that is to say, he argues for the elimination of the distinction between the highest political and the highest moral good and for the substitution of (Cohen's) "peaceful international order of democratic socialist societies" (p. 164). To be sure, this move depends on an equally serious depreciation of the role of individual, contractual rights, including obligations to political obedience arising from a prior, if "ideal," social contract.

Part 2 of *Kantian Ethics and Socialism* takes a provocative and rewarding look at the problem of morality in Marx in light of the ethical socialist tradition associated with Hermann Cohen and other members of the Marburg School. English-speaking readers will find this section of the book particularly valuable for its careful and sympathetic reprisal of a most serious (and neglected) contribution to socialist thought in this century. At the same time, the author's effort to combat moral terrorism and reverse socialism's fatal substitution (via a Hegelianized Marx) of historical necessity for moral law seems itself threatened by the utopian Charybdis against which Marx railed. In reconstructing the moral law in the light of his own humanitarian hopes, Van der Linden runs the risk of abandoning along the way the transcendental freedom (with its insistence on the radical disjunction between nature and morality) Kant thought the law required. Without it, hope may not be enough.

Nevertheless, this is a carefully argued and provocative book that merits the serious consideration of Kant scholars and others interested in the moral foundations of socialism.

Susan Shell

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Peasant-Citizen and Slave: The Foundations of Athenian Democracy. By Ellen Meiksins Wood. London: Verso, 1988. $40.00.

Wood attempts to defend what may have been the only worker democracy in history—Athens—from its detractors, both ancient and modern. She seeks to show that contrary to the impression Aristophanes gave in the *Wasps*, Athenian citizens never supported themselves solely or even primarily with payments for public service. Most were peasants who served the polis in the agricultural off-season, or artisans. They did not live off slave labor, as most Marxists have argued.

The evidence about the organization of the productive system underlying ancient Greek poleis is admittedly scant. All claims about it are, therefore, fundamentally speculative. But Wood argues that what evidence there is suggests slaves were used mainly for personal service in the homes of the wealthy and in production aimed primarily for export, for instance, the silver mines. Indeed, slave labor was used in these sectors of the economy because, after Solon's reforms, small landholders and artisans were not legally or economically obliged to undertake it.

Athens has been severely criticized recently for its complete exclusion of women from public office and property rights. Not wanting to defend discrimination on the basis of sex, Wood nevertheless points out that not allowing women to inherit the family property prevented the kind of concentration of wealth that developed in Sparta. Landholdings (and hence the primary economic basis of Athenian democracy) remained, as a result of the exclusion of women from holding property, much more egalitarian than in Sparta or Rome.

Partly because the actual evidence is so scant, Wood argues, modern analyses of Athenian democracy have always reflected the political proclivities of the historian as much as the bare facts. Until the late eighteenth century, both admirers and critics of Athens agreed that the demos was composed primarily of farmers, artisans, and workers. That consensus gave way to the belief that Athenian democracy rested on slave labor as a result of the work of Thomas Mitford. Defending the balance of power represented by the British Constitution from radical egalitarian critics in France, Mitford tried to show that the ancient democracy the French praised so highly was not only plagued by severe class divisions but was also founded on slavery. Liberals like George Grote, who praised Athenian democracy in response to its conservative critics, tended to ignore the existence and issue of slavery altogether. Following Hegel, most Marxists then argued that the Athenian polis was not truly democratic because it was based on slave labor.

Wood's own analysis certainly supports her general observation about the political roots and consequent limitations of modern histories of ancient Greece. Arguing that Plato's and Aristotle's criticisms of the participation of farmers and mechanics in the deliberative process indicate that that was the actual practice—which these aristocratic philosophers were trying to overturn—she misses the substance of their objections entirely. She seems to think that their reservations about Athenian democracy can be dismissed merely by observing that they had aristocratic sympathies and wished to reinstitute hierarchy. From her account, no one would have the slightest idea that Athenian democracy was unstable or that the demos had difficulty retaining and rewarding reliable leadership. Seriously downplaying the importance of Pericles' reforms and the Peloponnesian War, she treats the regime as if it remained basically the same from Solon's reforms through the conquest of Athens by Sparta. Any reader of *The History of the Peloponnesian War* would have difficulty accepting Woods's hypothesis that the Athenians more collected tribute to build ships than built ships to gain tribute. As the Melian debate dramatizes, what had originally been a defensive league led by Athens developed into a tyrannical Athenian empire. Wood refers to Thucydides' history only once, when she disputes the inferences Ste. Croix draws from his report that 20 thousand slaves escaped from Attica during the Spartan occupation of Decelea. Apparently committed to the proposition that a democracy composed of small landholders and artisans is desirable, in and of itself, she is not interested in strictly political questions of constitutional change, responsible leadership, or foreign policy (or its interrelationship with domestic politics). She is concerned, to the exclusion of almost everything else, to show that the socioeconomic basis of Athenian democracy was not as inegalitarian as some critics have claimed.

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AMERICAN POLITICS

Consumer Safety Regulation: Putting a Price on Life and Limb. By Peter Asch. New York: Oxford University Press, 1988. 172p. $24.95.

This books written by an economist tries to show both the uses and limits of economic analysis for assessing consumer safety regulation. The author states that he is speaking to those with a general interest in the topic (including policy makers) and that he is trying to provide a bridge between proregulatory consumerist tracts and the often critical writings of economists.

The first chapter provides a good description of why people disagree about policies: they differ in their assessments both of the ability of consumers to make good decisions and of the strength of the nonregulatory incentives encouraging firms to make safe products.

The book reads more like a textbook for a course on the economics of consumer safety regulation than like an analysis of issues for policy makers or the general reader. As a text, the discussions of market failures, the economics of information, and recent research undermining the assumption that consumers are expected-utility maximizers are all nicely presented. Asch's approach is balanced and thoughtful. He cogently, if repetitiously, explains to noneconomists why "total safety" is neither attainable or desirable. After focusing on the appropriate rationales for regulation, he provides brief reviews of research on the costs and benefits of selected federal regulatory measures dealing with auto safety, foods and drugs, and other consumer products. Students encountering the field for the first time, including those with no economics background or only an introductory course, should find the book helpful.

Scholars, however, will find nothing new here. There are no original contributions either in empirical assessments of policy impacts or about the design of new policy.

The absence of the last indicates that policy makers, too, will gain little beyond an appreciation for the limits of ideological approaches to policy making in this area. The author's inconclusiveness about policy accurately reflects much of the writing about product safety regulation. People realize that the model of the rational consumer is flawed, yet, as Asch notes, "evidence that people choose inconsistently, while intriguing, points to no clear policy statement. Should we undertake programs that will encourage or require 'more consistent' consumer behavior?" (p. 83). Because conclusive answers to that question are elusive, he falls back, like others, on reviewing what is known about the costs and benefits of different regulatory actions.

The central issues for policy research include the following: How does consumer behavior change in response to different safety information strategies? What is the most sensible way to make a normative comparison of information strategies with more intrusive regulatory strategies? Researchers are just starting to look at the first issue. See, for example, W. Kip Viscusi and Wesley A. Magat, *Learning about Risk: Consumer and Worker Responses to Policy Information* (1987). Another interesting recent work, *Consuming Fears*, edited by Harvey M. Sapolsky, examines the politics of consumer safety, a topic Asch devotes little attention to.

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The Post-Modern Presidency. By Ryan J. Barilleaux. New York: Praeger, 1988. 175p. $35.95.

Barilleaux's central theme is that the presidency of the late twentieth century has entered a new era in its evolution, substantially different from the office as we knew it just 20 years ago. Though the reshaping of the presidential office has roots in the Nixon, Ford and Carter administrations, fundamental changes in the characteristics of the office were consolidated by Ronald Reagan in a fashion that gave them lasting significance.

The transformation to what Barilleaux calls the postmodern presidency was precipitated in large part by changes in the political climate of the nation. The New Deal era, which reached its climax with Lyndon Johnson's Great Society, had achieved a dominant role for the federal government by creating a host of government programs designed to improve the general welfare of the citizenry. But this era of growth in the federal government was not without its faults. It drew attention to questions of the federal government's effectiveness in managing social programs. Concerns about governmental waste and efficiency took center stage with four successive presidents, from Nixon to Reagan. (One might add as a postscript that in the presidential campaign of 1988, questions of competence in managing the federal government were once again elevated over the rhetoric of governmental expansion and activism. As former governor Charles Robb of Virginia suggests, "the political era that was dominated by the New Deal and everything that has flowed from it has run its course" [p. 62]).

Barilleaux asserts that the change in political climate between the Roosevelt and Reagan eras brought with it six discernible and distinctive characteristics: (1) the revival of presidential prerogative power; (2) governing through public politics; (3) the development of a general secretariat to serve the president; (4) the rise of vicarious policy making; (5) the president as chief whip in Congress; and (6) the new vice-presidency (p. 8).

Barilleaux's discussion of the rise of prerogative power as a "fundamental feature" (p. 83) of the postmodern presidency illuminates the contemporary context of the office. Building on Richard Pious's (*American Presidency* 1979), he argues that presidents must resort to prerogative power with increasing frequency in order to "break through the apparent logjam of 'separate institutions sharing power' " (p. 83). In foreign affairs, for example, the use of "parallel unilateral policy declarations" by presidents Carter and Reagan kept the SALT II treaty alive for some eight years by sidestepping the constitutionally prescribed process of Senate ratification, where defeat of the treaty appeared imminent.

Prerogative power in the postmodern era is not confined to foreign affairs but extends to the realm of domestic policy making as well. For example, Ronald Reagan, by executive order, instituted mandatory review of all "major" rules promulgated by federal agencies, "prescribed a system of cost/benefit analysis that each proposed rule had to survive in order to be published, and required each agency to establish a regulatory agenda that would detail all regulatory activity either 'planned or underway' " (p. 86).

Beyond the rise of prerogative power, one of the fundamental characteristics of the postmodern presidency, according to Barilleaux, is the permanently altered landscape of executive branch organization. In this regard, the author sets forth four major propositions: (1) that "a big and active presidential establishment is inevitable" (p. 107); (2) that the nonhierarchical White House is, or at least should be, a thing of the past; (3)—a corollary to the second point—that the appointment of a White House chief of staff is now essential, notwithstanding Jimmy Carter's refusal to designate such an official during the first two years of his administration; and (4) that "It is time to put to rest the myth of 'Cabinet government' " (p. 108).

The first and fourth propositions run directly counter to the earlier arguments of Stephen Hess's *Organizing the Presidency* (1976). And despite the changing complexion of the presidential office in the intervening twelve years, I have recently defended Hess's earlier views on Cabinet government and on the need to place limits on the size of the presidential establishment, while finding common ground with Barilleaux on points 2 and 3 (*Managing the Presidency: The Eisenhower Legacy* [1988]).

Barilleaux readily concedes that some of the characteristics that he ascribes to the postmodern presidency differ in degree more than in kind from earlier trends in the development of the office. Still, some of the areas under discussion deserve more attention with regard to broader historical patterns of change. Is the relationship that currently exists between the president and Congress long-term in complexion, or is it passing through a stage in the cyclical ebb and flow of power postulated by Lawrence Dodd (Dodd and Oppenheimer, *Congress Reconsidered* [1977], 269–307)? If the current state of congressional decentralization of power is transitory, as Dodd's thesis would suggest, such postmodern presidential roles as "chief whip of Congress" may also be transitional.

In any case, *The Post-Modern Presidency* provides a useful and well documented guide for understanding the contemporary office. The book's major contribution rests with its examination of recent changes in the office of the presidency and their consequences for future presidents. While other studies have looked at aspects of these changes in isolation, this volume integrates recent developments in a coherent and convincing fashion.

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Understanding United States Government Growth: An Empirical Analysis of the Postwar Era. By William D. Berry and David Lowery. New York: Praeger, 1987. 232p. $39.95.

One of the aspects of public policy most taken for granted yet least understood is the growth of government. In this book, Berry and Lowery take us a considerable distance toward a better understanding of it.

Using econometric analysis of annual time series data for the period from 1948 to 1982, the authors evaluate and compare a variety of theories about the causes of government growth. Very broadly, they attempt to distinguish a *responsive government* interpretation, which sees growth as the result of changing objective conditions and popular preferences, and an *excessive government* interpretation, associated with conservative economists and public choice theorists, which sees growth as the result of self-interested behavior by politicians and bureaucrats. They find far more support for the responsive government view.

In two very useful preliminary chapters, Berry and Lowery provide a critical review of the existing literature on government growth and discuss some problems of measurement in this research. The excessive government theories they review hold that growth results from the electoral power of government employees, public misperception of the costs of government ("fiscal illusion"), bureaucratic monopoly of information, a progrowth bias in election years, interest group political pressure, and decentralized intergovernmental structures that separate spending and the raising of revenue. The responsive government theories hold that growth results from certain aspects of industrialization such as increasing national income (Wagner's Law), macroeconomic responsibilities associated with the growth of trade, economic changes that have made a larger share of personal income subject to taxation, changes in public preferences expressed through the election of the more liberal (Democratic) party, a ratchet effect of wars and crises, and developments in communications that have increased awareness of economic inequality. Berry and Lowery give a cogent appraisal of the logic and evidence supporting each of the theories.

Regarding measurement, they demonstrate that the measure of government size employed in most of the research—government expenditures in current dollars as a proportion of gross domestic product (GDP)—is inappropriate for testing these theories. Surprisingly, most of the growth in that measure in the postwar era is accounted for by a steady increase in the cost of government services relative to costs in the private economy, attributable to greater productivity growth in the private economy. With relative costs held constant, the level of government activity has approximately kept pace with the growth of the private economy. Moreover, government transfer payments have grown at different times than government purchases (including salaries), and domestic purchases at different times than purchases for defense.

As a result of using the aggregated, current-dollar measure, theories have been tested with data that they should not, in principle, have been able to explain. For example, theories that purport to explain why government services expand have found support in growth data that in fact mainly reflect public sector inflation. Theories that emphasize the power of government employees have found support in data that mainly reflect increases in the size of transfer payments with no direct benefits to such employees. In a revealing preliminary analysis of their data, the authors show that none of the leading theories of government growth performs well in explaining the movements of the government's share of the economy in constant dollars.

To improve on this unsatisfactory situation, Berry and Lowery perform separate analyses of the growth of relative costs, transfer payments, domestic purchases, and defense purchases. In each case, they identify the theoretically relevant causal variables (for example, using government employees' electoral power as a potential explanation for purchases but not for transfer payments), use them to construct both a responsive government and an excessive government econometric model, and compare the performance of the two models. They operationalize a few of the variables somewhat arbitrarily. (For example, Wagner's notion of increasing interdependence resulting from economic differentiation becomes a simple matter of the age structure of the population). On the whole, however, they do a careful and intelligent job of constructing the models and performing the tests. Only in one area, transfer payments, do they find any support for the excessive government view, and even there only for a relaxed version of it. In contrast, the responsive government models perform well in explaining every component of government growth.

The study's main weakness is the attempt to categorize all the models and explanatory variables on the basis of a grand distinction between a responsive government and an excessive government theory. In fact, there are several distinct theoretical issues: whether the demand for growth comes from within or outside of government; whether the occurrence of growth is associated with change within or outside of government; whether politicians and bureaucrats pursue self-interested or social objectives; and whether government is or is not excessively large in relation to some normative standard. Each of these issues is largely independent of the others, and it is not helpful to constrain theorizing by assuming tight linkages. One effect of this dichotomous thinking—which reflects the prominent ideological flavor of this entire body of literature—is some confused hedging about "extreme" and "less extreme" versions of the excessive government theory.

Notwithstanding this difficulty, Berry and Lowery's cogent treatment of measurement issues and their careful, disaggregated empirical analysis make this study a substantial advance over previous work on government growth.

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Hard Judicial Choices: Federal District Court Judges and State and Local Officials. By Phillip J. Cooper. New York: Oxford University Press, 1988. 374p. $34.00 cloth, $15.95 paper.

U.S. courts have been under a continuous barrage of criticism by conservative academicians, judges, and politicians since the 1960s. During the Reagan era, the administration fueled the flames of the controversy over the proper role of courts and judges in the U.S. system of government. That debate reached a level of intensity and ideological polarization not seen since the 1930s and played a part in the defeat of Robert Bork's nomination to the Supreme Court in 1987. The controversy has been framed variously as that over judicial activism versus restraint, interpretivism versus noninterpretivism, jurisprudence of original intention versus the jurisprudence of the core values of a living Constitution, the essentialist view of the role of courts versus the adaptationist view, and the question of judicial capacity.

When the issue is framed in terms of judicial capacity, the questions raised are whether judges have the competence to deal with complex issues of social policy and whether courts should issue remedial decrees whose implementation and supervision may exceed their administrative and political abilities. The discourse of judicial capacity has been marked by heated rhetoric (e.g., charges of an "imperial judiciary"), although sometimes there have been thoughtful highly critical analyses, (such as Donald Horowitz' *Courts and Social Policy).* Into this battle now comes Phillip Cooper's important book, *Hard Judicial Choices,* a work of meticulous scholarship that will have to be confronted by those on all sides of the judicial capacity and related controversies.

Cooper offers five detailed case studies of the hard judicial remedial decree choices faced by federal district judges in cases concerning civil liberties or civil rights issues in activities of state or local government. In each instance, the judge was faced with a proven violation of statutory or constitutional law. For each issue, Cooper provides a background chapter briefly discussing the broader problem, a capsule constitutional and statutory history (where appropriate), and the relevant case law. This is followed by a consideration of the particular facts of each of the five cases, the decisions of the judges, and what happened afterwards. The story of what happened after the initial finding of liability is only partly about what happened on appeal. (The appeals process is found by Cooper to be "a parallel process within the remedy crafting stage" [p. 347].) It is mostly about the interaction of judges and state and local officials within the context of efforts to fashion remedies that effectively dealt with the conditions that stimulated the litigation.

The case studies include the issue of racial discrimination in housing (*United States* v. *City of Parma, Ohio)*, school desegregation (the famous Detroit metropolitan area case, *Milliken* v. *Bradley)*, the right to treatment in state mental health facilities (the Alabama case, *Wyatt* v. *Stickney*, that brought nationwide attention to Judge Frank Johnson), overcrowded and unlivable prisons (the *Rhodes* v. *Chapman* case), and police violation of individual rights (*Rizzo* v. *Goode)*.

In the introductory chapter, Cooper tells us "I will not sit in judgment on the judges by declaring whether they should or should not have issued the orders in question" (p. 9). The issue, he suggests implicitly in the body of the book and explicitly in the conclusion, is not that of judicial capacity: The judges "simply are not in a position to refuse to respond to proper cases instituted by appropriate parties under provisions of statutory or constitutional law" (p. 328). Instead, using his model of decree litigation, Cooper in his concluding chapter skillfully uses his case studies to suggest various points in the process in which judicial choices can be more effective.

Cooper's determination, however, to view the five cases from the perspective of the district judges and his unwillingness to be judgmental of judicial behavior (from a policy analysis perspective) leaves him open to the charge that he was reluctant to make hard *scholarly* choices. In the three cases decided by the U.S. Supreme Court, the Court overturned the lower courts. There is only little discussion of why and of what that implies about the behavior of district judges. Likewise, judicial role analysis is slighted. I would have liked more attention given to placing the findings within the context of the judicial behavior literature. Important findings and conclusions are left to the last chapter and are not given the emphasis they deserve. (For example, Cooper's concept of "the deference paradox" should have been employed as a major theme in the case studies).

Nevertheless, taken on its own terms, this is a book that is destined to become a classic in the judicial administration literature. This is a book for the classroom as well as for judicial politics specialists. And this is a book that provides grist for the mills of those involved in the debates concerning the role of courts.

SHELDON GOLDMAN

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Business, Money, and the Rise of Corporate PACs in American Elections. By Theodore J. Eismeier and Philip H. Pollock III. New York: Quorum Books, 1988. 122p. $35.00.

Corporate political action committees (PACs) are more diverse than commentators commonly acknowledge. And "their behavior is . . . affected by a complex set of internal and external forces" (p. 9). Those, at least, are the assumptions that underlie this book.

The authors identify as important variables in corporate PAC behavior the financial size of PACs, their degree of organizational centralization, the extent to which they are ideologically predisposed or politically pragmatic, their headquarters location (in Washington or elsewhere), their responsiveness to appeals from "bellweather PACs" and party organizations, the "climate of partisan expectations" in each election year, and the degree and character of government regulation of their parent corporations.

Much of the authors' analysis previously appeared in their articles and papers. Some of the analysis repeats others' work, generally arriving at similar conclusions. Data presentations are often unnecessarily obtuse, although finally comprehensible after detailed study of the text. Perhaps most puzzling is the authors' failure consistently to specify the financial size—and thus the potential contribution and expenditure capacity—of the corporate PAC subsectors they analyze, even while specifying the number of PACs in each subsector.

In several respects, nonetheless, the authors advance our knowledge of the varied world of corporate PACs. In traditionally regulated industries where regulatory benefits tend to be concentrated in regulated firms and costs are widely dispersed among the public, corporate PACs generally pursue "accommodationist" political strategies by contributing very heavily to incumbents, especially party leaders and senior committee members, of both parties. But PACs of corporations most heavily affected by the more recent "social regulations" of the 1970s (e.g., Occupational Safety and Health Association, National Highway Traffic Safety Administration, the Surface Mining and Reclamation Office) that concentrate costs on industry while dispersing benefits across broad and relatively unorganized publics engage in the "entrepreneurial" politics of supporting mainly probusiness Republicans, including many who are challenging Democratic officeholders. Moreover, entrepreneurial corporate PACs are much more ideological: they emphasize contributions to incumbents whose voting records are at the conservative end of the congressional spectrum. Finally, these entrepreneurial PACs are disproportionately located in the South and West, where the Republican Party has most firmly established its modern support base and has, also, been most conservative.

The authors confirm earlier studies showing that the overwhelming number of corporate PACs are quite small. About half have campaign contributions budgets under five thousand dollars, contribute to 10 or fewer candidates, and make average contributions of less than five hundred dollars. While many larger PACs contribute in response to external cues from "bellweather PACS" and national party leaders, small corporate PACs are overwhelmingly "hinterland homebodies," who direct most of their contributions to in-state congressional candidates.

Confirming earlier reports, the authors find that in 1980 corporate PACs took a highly partisan approach, extensively supporting Republican challengers over Democratic incumbents. Republicans won a majority in the Senate and made gains in the House of Representatives. This PAC strategy did not persist: to defend Republican incumbents, meet strong "demand side" pressures from Democratic congressional leaders, and respond to a climate of expectations that Democrats would run well, corporate PACs in the years since 1980 have contributed most generously to Democratic and Republican incumbents while giving secondary levels of support to Republican challengers. If Democrats become less well entrenched in Congress, however, PAC money will undoubtedly shift dramatically to Republican incumbents and especially to Republican challengers.

Generally sympathetic to their subjects, the authors show some unease about the role of corporate PACs in U.S. politics. First, they acknowledge more readily here than in an earlier paper the data demonstrating that PAC gifts may weaken the links between congressmen and their districts, by creating a second, out-of-district financial constituency for officeholders. Despite the "homebody" behavior of small corporate PACs, fully 75% ($17 million) of all corporate PAC gifts in House races in 1984 went to out-of-state candidates.

Second, the authors express concern that "candidates' incessant demands for funds have made managers of even the largest Washington PACs feel more like the hunted than the hunter (p. 101)." Just such high pressure fund-raising tactics by the executive branch led to Watergate and then to sweeping reform of presidential campaign finance. The authors implicitly acknowledge this problem: they recommend a high aggregate limit—perhaps 180 thousand dollars—on all PAC contributions to a single House candidate but with no aggregate limit on candidate expenditures. This would simultaneously diminish the concentration of PAC contributions on a few candidates while respecting the opportunities for challengers to raise and spend campaign funds.

Despite their reluctant concession that some reform may be needed, the authors never grapple with large issues of corporate PAC participation in politics. Should corporations and similarly artificial "persons" who are not voters be authorized to play such large roles in the electoral process? Does the formal entry of institutional wealth into electoral politics jeopardize the delicate balance between a free market economy that encourages the aggregation of capital and a democratic polity that emphasizes approximate equality for citizens in decision making?

Amidst a torrent of recent books and articles on corporate PACs, this volume is a useful additional empirical contribution. It makes only a modest contribution, however, to fixing the role of corporate PACs in our larger political and economic system or in evaluating them against some vision of democracy.

DAVID ADAMANY

Wayne State University

In Search of Safety: Chemicals and Cancer Risk. By John D. Graham, Laura C. Grew, and Marc J. Roberts. Cambridge: Harvard University Press, 1988. 260p. $35.00.

Searching for Safety. By Aaron Wildavsky. New Brunswick: Transaction Books, 1988. 256p. $32.95 cloth, $16.95 paper.

Public debate about risk is strewn with clichés: we are not safe, accidents are inevitable, everything causes cancer. The fears expressed pertain both to new technologies, which always pose unpredictable risks, and to revelations about hitherto-unforeseen dangers in activities once thought benign. Indeed, contemporary life itself seems more perilous than ever. Within the boundaries constructed by these perceptions lies the dilemma of public policy making, for policy makers and citizens alike are faced with the problem of deciding which—or what level of—risk is acceptable and which is not. The dilemma is, in essence, to judge how safe is safe enough.

Two new books, in different ways, address these related questions of safety and risk. But as Aaron Wildavsky suggests, the debate over safety too often is one-sided, rhetorically dominated by those seeking above all to eradicate existing risks and avoid new ones. This he sees both as irrational (for it is impossible to avoid all risk) and as counterproductive (for a society seeking to avoid all risk may in fact miss new opportunities or benefits that can be gained from taking chances. Graham, Grew, and Roberts, for their part, worry that the role of science in risk assessment (in this case, evaluating potential carcinogens) is misplaced, that scientists and their studies are ill-understood, ill-used, and, in the end, probably oversold to a public increasingly convinced that the world is more dangerous than ever.

These works dovetail nicely, both addressing common fears about the risks society courts everyday. Wildavsky, in examining a wide range of cases (including nuclear power and the U.S. system of torts) seeks above all to redefine the arguments about safety in a provocative and thoughtful response to those for whom all risk is to be avoided. Graham and his Harvard colleagues, for their part, deliver a thorough and enlightening evaluation of how policy makers use science in regulating formaldehyde and benzene. The studies address the debate about safety and risk at different levels of analysis, but in the end each finds the current discourse lacking, and even wrong.

Wildavsky is concerned that prevailing conceptions of safety may in the long term harm the society as a whole. Risk, he reminds the reader, is a mixed phenomenon, always producing both danger and opportunity. Too often, however, the debate revolves purely around some potential danger, which he argues, too often centers on low-probability, high-impact incidents (e.g., nuclear meltdowns or runaway genetic mutations). Such a one-sided "search for safety" may in fact result in a blanket refusal to take any risks: but risks, Wildavsky asserts, are necessary for successful technological change, economic growth, and, ultimately, the overall resiliency and health of the society. Risk taking is critical to adaptation: it produces more opportunities, more resources, and, most important, more safety, since wealthier societies actually can absorb the impact of great disasters more easily than can poor ones.

What worries Wildavsky most is an emerging philosophy, best exemplified by Jeremy Rifkin, that no new technologies should be introduced—or even tested—without absolute prior proof that the technology poses no risk. But, Wildavsky argues, this is impossible. There can be no safety without risk, and playing it safe actually can reduce opportunities to benefit from any chances taken. Such a view is based on three compelling premises: we simply know too little about the possible consequences of present acts to reduce uncertainty totally; the good and the bad, safety and danger, always are intertwined; and, finally, some risks to some parts of any system must be taken for the system as a whole to be able to adapt to change.

A society that avoids taking any chances robs itself of potential benefits. It is better, Wildavsky argues, to adopt a strategy of incremental trial and error, to experiment with many small risks, than to try to anticipate all potential danger. A strategy of risk avoidance at all cost eats up critical resources and ultimately undermines societal resiliency, producing in the end (to echo Mancur Olson) societal sclerosis and decline. After all, as Wildavsky points out, joggers routinely risk heart attacks to improve overall health, so it is better for a society to run short-term risks in order to expand future wealth and health. The trick, he concludes, is to discover not how to avoid all risk, but how to use it to get more of the good and less of the bad. The search for safety, thus, is and must be a balancing act.

Graham, Grew, and Roberts pursue less sweeping ends. In examining scientific and political disputes over the potential carcinogenic dangers posed by formaldehyde and benzene, the authors find, in conclusions perhaps not overly surprising to students of public policy, that nontechnical considerations play critical roles in setting priorities when experts are in sharp debate over data and methodologies. Institutional biases, ambiguous legislative mandates, political agendas, bureaucratic routines, mushy standards, and so on lead the authors to suggest, without really stating it plainly, that where you stand on the safety of some types of potential carcinogens often depends on where you sit.

Having implied this, Graham and colleagues argue strongly that regulatory risk assessment has become too formalized and overly mechanistic in light of limited data, problems in extrapolating from the data, and the low degrees of confidence scientists express in their findings. The experts may understand the shortcomings of their studies, but they nonetheless cave into pressures generated by policy makers or their own research agendas to come up with definitive answers. Not surprisingly, the research findings presented may actually exacerbate policy conflict as much as it resolves disputes, frequently to the surprise and dismay of those involved and to the bewilderment and cynicism of a public that is increasingly unsure whether anything is safe.

Above all, the authors argue, the capacity of science and scientists to resolve sharp policy conflicts is oversold. The main problem is that it is far easier to raise alarms about potential dangers than to prove that any technology poses no harm, an asymmetry that buttresses nicely Wildavsky's attack on those who would avoid all risks. This said, Graham and his colleagues argue that scientists and policy makers alike must make risk assessment more flexible. Uniform guidelines (e.g., the Carter administration's cancer policy) are to be avoided because they drape the mantle of scientific precision over honest disagreements about methods and data. Above all, they conclude, it is necessary for scientific and political values to be juxtaposed openly, and for accountable decision makers to stop hiding behind the false front of scientific consensus and actually make political decisions.

These studies thus express similar concerns. Wildavsky, a well-known skeptic of planning and defender of incrementalism, believes that those who seek absolute safety may actually undermine the very wealth that allows us to worry not about poverty or hunger but about how many parts per billion of ethylene dibromide poses real danger. Graham and colleagues do not go so far in their conclusions but do worry that the debate about cancer has been distorted by misperceptions about and the misuse of science in regulatory decisions. Both studies find the same asymmetries in the public debate about risk, and both advocate greater openness and flexibility in judging safety and its costs.

Wildavsky, however, might be accused of insensitivity to real harm experienced by real people, of caring more about the society than about those who actually might get hurt. Indeed, it would be easy to distort his views to make him appear concerned simply about economic growth over human health, but such charges would be unfair. Wildavsky argues forcefully that when the danger is clear and the solutions are known, risk avoidance is preferred to trial and error. But, he rejoins, such certainty is rarer than we might admit: so in most cases it is better to experiment with small risks—be they tests of genetically engineered organisms or new types of toxic waste dumps—for the greater good of the society. This is, above all, a polemic (against those for whom all risk is unalloyed evil), but it is worth careful consideration.

Graham, Grew, and Roberts could similarly be accused of naiveté, of calling for more reasonable use of science (and scientists) by policy makers, a partial return to judicial deference to regulators' expertise, and more courage on the part of elected officials. Yet one should avoid the temptation to snicker at these conclusions, for the authors present a modest and compelling call to scientists to be more honest in their presumptions and for policy makers to do their jobs. If it is naive to argue for a more open debate about the acceptability of some types of risks, or to question the propensity of the current discourse to degenerate into the everything-causes-cancer syndrome, so be it.

Wildavsky's work, written consciously for a broad audience, is the more accessible of the two. It is provocative and thoughtful and would be very useful for both graduate and upper-level undergraduate courses in policy studies. Graham and colleagues often get highly technical, since much of the study examines competing scientific methodologies and judgments about the nature of cancer itself. One suspects that the authors intended above all to reach fellow scientists and academics, but it can be useful in some advanced graduate seminars. Such differences aside, both works remind us that the world of risk and safety is messy and unpredictable, so both are worth reading.

CHRISTOPHER J. BOSSO

Northeastern University

On Account of Sex: The Politics of Women's Issues, 1945–1968. By Cynthia Harrison. Berkeley: University of California Press, 1988. 337p. $25.00.

Historians of women have conventionally viewed the years between 1920 (the adoption of universal woman suffrage) and 1966 (the formation of the National Organization for Women [NOW]) as largely devoid of feminist activity. This interpretation is currently undergoing significant challenge and revision. In *Survival in the Doldrums: The American Women's Rights Movement, 1945 to the 1960s* (1987), Leila J. Rupp and Verta Taylor documented the persistence and continuity of the movement throughout this century. In *On Account of Sex,* historian Cynthia Harrison provides a complementary history of women's issues during four presidential administrations, Truman to Johnson.

The fact that women's policies were adopted and aggressively implemented during 1961–63 suggests that important policy changes can occur given an appropriate political and social setting and capable leadership even if no mass-based social movement demands them. Harrison further argues that these pre-1966 federal initiatives played a crucial role in the development of the current women's movement by breaking a critical stalemate over goals among women's groups, providing legislative tools, setting the women's agenda for a period, and legitimating and nurturing a self-interested women's organizational network.

Scholars of women's role in U.S. politics will find this study an invaluable resource. For example, Harrison's extensive archival research turns up several fascinating but little-known historical facts: how "comparable worth" language was dropped from the Equal Pay Act of 1963 (EPA); the genuinely pro–women's rights sentiments of Representative Howard Smith, the author of the prohibition against sex discrimination in Title VII of the Civil Rights Act of 1964; and the extremely tenuous (and, in one case, forged) endorsements of the Equal Rights Amendment by Presidents Truman, Eisenhower, Kennedy, and Johnson. Activists, too, will easily draw parallels between the Reagan years and the period surveyed here, an era when rigid gender roles served a number of national functions and the appointment of women to visible policy positions was first used by presidents to avoid more substantive responses to women's issues. The case study presented here further explicates the relationships between environments, social movements, and policy changes.

But the readership of this book should not be confined to those specializing in gender politics and social movements. The interactions between the Women's Bureau and its coalition of social feminist groups is a fine example of bureaucratic clientele relations so often the focus of public administration research. The politics of presidential appointments of women and the formation (in 1961) and accomplishments of the President's Commission on the Status of Women (PCSW) provide new insights into the institutional presidency. Congressional scholars will find well-developed legislative histories of the EPA and other women's issues. Policy scientists are provided with additional proof of the long gestation periods often required for policy innovations. (For instance, an Equal Pay Act was first introduced in 1945 and a commission to study the problems confronting women had been supported by the Women's Bureau coalition since 1946.) Also of interest to policy specialists will be the switch from the inside access model of agenda building on women's issues to one of outside initiative; with the formation of NOW, the federal government had to respond to demands sustained by a high degree of public awareness instead of initiating moderate actions in the calm of general disinterest. The role played in presidential administrations by the heads of the Democratic and Republican National Committees' Women's Divisions, as described here, well illustrates both the limitations and potentialities of party office.

The historical narrative here is presented in a clear, straightforward style. Although Harrison draws on much of the standard political science literature on institutions and processes, she chooses to provide insightful analysis rather than to frame her study in terms of a particular social theory. Some will no doubt view this as a weakness. For others, the prodigious research effort needed to "get the story right" will more than compensate for the lack of explicit theory building. (In addition to interviews conducted by the author, 72 manuscript collections and numerous oral history projects in more than a dozen metropolitan areas were consulted.) In sum, this is a carefully argued, superbly documented, and much-needed work of original scholarship.

JANET K. BOLES

Marquette University

The First Charity: How Philanthropy Can Contribute to Democracy in America. By Robert Matthews Johnson. Cabin John, MD: Seven Locks, 1988. 252p. $13.95 paper.

Bridging the fields of philanthropy, community organization, and political science, Bob Johnson's book serves well as a clearly written, well organized, and compelling case for, first and foremost, funding community organizations and their key role in building and widening participation in the local political process.

The First Charity begins with discussions of alienation in communities, weaknesses and limitations inherent in pluralism, and the values of participation in political decision making and democratic politics. With that base, and having identified a need to encourage organization among the unorganized and participation among the unparticipating, Johnson reaches into his own experience as a funder-participant and provides a thorough, sensitive, and insightful treatment of the characteristics, strategies, and activities associated with successful, thriving, and participation-building community organizations. His observations and illustrations are grounded in, and explained through, rather detailed case studies of three successful community organizations—BUILD (Baltimoreans United in Leadership Development), MOP (Metropolitan Organizations for People, Denver), and UNO (United Neighborhood Organization of Southeast Chicago).

Johnson, having argued the need for, and value of, community organizations and participation building, next turns to the role and response of philanthropy. Historically, in Johnson's view, philanthropy, donors and foundations have not funded the unorganized, permanently poor who have needed economic assistance and support in building institutions and participation among themselves. Instead, philanthropy has expected that government will fund the needs of the poor while private donors and foundations direct their giving toward funding peers and self-serving causes through established institutions and professional authorities (museums, hospitals, schools, etc). While the poor may benefit indirectly from this charitable giving, that benefit accrues primarily through the so-called "medical model," in which professionals solve people's problems for them or prescribe solutions rather than fund or encourage people's capacity for becoming involved in problem solving and organization building for themselves. In Johnson's view, neither philanthropy nor government have supported the need for leadership development, neighborhood empowerment, and building participation in the political process.

Finally, Johnson's case for his *first charity* emerges from his groundwork in the fields of philanthropy, community organization, and the political process. He presents citizen involvment, participation, neighborhood organization, institution building, and leadership development as the most important, most fundamental first commitments for charitable giving (foundations, private donors). Beyond *simply urging that direction and commitment* for philanthropy, he sets forth the process for making it happen. The balance of his work then focuses on (1) encouragement and strategies for foundations getting involved with community organizing, (2) grantsmanship and proposal evaluation processes that are sensitive to the situations and needs of both funding sources and applicants, and (3) appropriate styles of management and consultant services available to community organizations.

Without diminishing the importance and contribution of Johnson's work, I think some of its limitations and assumptions need clarification. First, when he finishes building his foundation on the descriptive, empirical, and science-building work of the early pluralists and collective action theorists, Johnson goes further to portray them as normatively at odds with a positive approach to citizen involvement, participation, and community organization. Such portrayal seems unnecessary and, even if accurate, is not clearly relevant.

Second, while his case builds on the classics (Robert Dahl's *Who Governs?* [1961], Mancur Olson's *The Logic of Collective Action* [1971], Kenneth J. Arrow's *Social Choice and Individual Values* [1963]), Johnson seems to oversimplify or underutilize their otherwise rich theoretical implications. Moreover, his argument would benefit from the research and literature that has since emerged from those beginnings.

Third, the book makes, and implicitly builds on, two assumptions that need more explanation and discussion than they are given: (1) that participation building and community organizing are nonpartisan, politically neutral processes that pose little concern for donors over the redistribution of power and resources; and (2) that broadened, increased participation will lead to less poverty, environmental degradation, racism, and nuclear threat. Both could use more examination and use of data than the book offers.

Fourth, building on three case studies of "successes" does not enable much generalization or systematic analysis. Nor can we easily contrast successes and failures.

Fifth, although Johnson does establish community organizations, leadership development, and participation as important, cost-effective, beneficial objectives for philanthropy; he does not make the case that they have a *first* claim on donors' resources. We are *probably years away from being able to establish* logically or scientifically a philanthropic rule prescribing, for example, that giving to community building is more fundamental, important, or imperative than giving to food banks, childrens' hospitals, or universities.

Even considering these limitations, *The First Charity* is a needed and articulate contribution to our ways of thinking about giving, participating, and organizing. And if we accept it as an exercise more in case-building than science building, its impact will be as intended—a fuller, richer, more diverse philanthropic and political agenda. Philanthropy and politics need its perspective and urging. Community organizations need its encouragement and its commitment to empowerment.

DEAN SCHOOLER

The Development Center, Boulder

The Trusteeship Presidency: Jimmy Carter and the United States Congress. By Charles O. Jones. Baton Rouge: Louisiana State University Press, 1988. 225p. $24.95.

This book offers the most penetrating analysis to date of the Carter legislative presidency. Not only is the book a revealing portrait of what went both wrong and right with the Carter agenda, it provides a model for future presidential scholarship. Those who seek to explain presidential influence in Congress will find *The Trusteeship Presidency* an essential reference book.

The book itself reflects a careful blend of methodologies. Jones is above all a gifted storyteller, and readers will find his case studies of Carter's legislative successes easy to absorb. That does not mean, however, that the book lacks depth. Indeed, Jones has set a high standard for future research. Detailed interviews with White House and congressional staff, box scores, chronologies, case studies, secondary analysis, and primary documents all inform the book.

The materials are interwoven with skill, tied together into a book that refutes the conventional wisdom about Carter and Congress. Just as Fred Greenstein's *Hidden-Hand Presidency* (1982) opened a new period of research into Eisenhower, so will *The Trusteeship Presidency* give future students of Carter the essential first threads of their research.

The book itself is organized around one simple theme: Carter's legislative strategy was the natural product of his view of the role of the president as a trustee of the people. Carter believed in the fundamental rightness of his cause. He was an outsider by choice, not by accident. As Jones so clearly argues in chapter 2, "How Jimmy Carter Came to Washington," virtually every molding experience on Carter's road to the White House reinforced his distaste for the kind of high-speed bargaining that characterized Lyndon Johnson's legislative operation.

Carter was not the first president to champion a cause, of course, nor the first to agonize over political trade-offs. All presidents are torn between vision and immediacy; they are pols and prophets at the same time. Yet few eschewed politics like Carter. Not only did Carter believe in the rightness of his purpose, he believed in the rightness of his process—a process that left little room for congressional consultation, particularly since Carter viewed such input as unpleasant and unhelpful.

Yet as Jones documents in an excellent history of the modern Congress, Carter may have lost a rare opportunity to build a new institutional dialogue, clinging to his own anachronistic views of Congress in spite of changing realities. "The new President faced a new Congress," Jones writes, "one with changed attitudes about itself, fresh faces and new leaders, improved capabilities, and different ways of doing business" (p. 67).

Carter apparently did not take note, pondering instead whether a parliamentary system might be better for a trustee president. Jones does not address the issue in detail and may have missed an important opportunity himself in not doing so. Many of the Carter staff, most prominently former counsel Lloyd Cutler, have argued that the system, not the occupant, was to blame for the president's frustration. Jones's book seems to indicate otherwise.

Jones's book is far more than a discussion of the state of institutional relations in the post-Watergate period, however. It is also a study of how Carter both structured his congressional liaison operation and pursued his policy agenda. Among the many unexpected findings, for example, Jones rescues Frank Moore and the Carter legislative liaison operation from the scrapheap of congressional scorn. According to Jones, Moore and the liaison team did the best they could in trying to meld a legislative strategy to fit Carter's own views. "The president was Jimmy Carter," Jones writes, "and the liaison team was in no position to change that fundamental fact of political life" (p. 121).

Future liaison directors will find comfort in Jones's analysis. They will also discover important lessons on how to succeed. Indeed, as Jones's case studies of the 95th and 96th Congresses amply show, Carter was not without occasional victories. The Panama Canal Treaties and Civil Service Reform were surely legislative triumphs—the former coming over highly organized, often vitriolic opposition, and the latter over profound apathy and entrenched resistance.

Thus, it is not that the two gears of presidential-congressional relations can never mesh in the trusteeship presidency. Rather, as Jones makes clear, legislative success takes a very skilled hand and does not come easily to those who are as unfamiliar with the gearbox as Carter was.

PAUL LIGHT

University of Minnesota

Comparable Worth, Pay Equity, and Public Policy. Edited by Rita Mae Kelly and Jane Bayes. New York: Greenwood Press, 1988. 280p. $49.95.

The essays included in this volume make, on the whole, an important contribution to scholarship in employment policy generally and to the understanding of comparable worth specifically. The major limitation of the volume is the failure of the editors to seize the opportunity to use the essays as the basis for a more cohesive and coherent statement.

Although the terms *comparable worth* and *pay equity* are commonly used interchangeably, the editors have defined the scope of the book to be broader than comparable worth, which refers to the elimination of gender-based pay disparities among different jobs within the same employing unit. The editors pursue the general concern of gender-based economic inequities in the workplace that contribute to the overall gap in earnings by men and earnings by women. *Pay equity* refers to this general concern. The volume pays special attention to comparable worth but, in a rather disjointed way, also touches on a variety of other issues.

Among the best essays in this collection is the articulate and insightful review of labor market theories by coeditor Jane Bayes. Her presentation effectively explains the complexities of wage setting and cogently shows patterns of gender-based pay disparities within and between various occupations and industries. The value of this essay would have been enhanced by including historical evidence documenting conscious decisions to establish sex-segregated occupational groupings and to pay the female occupations less on the assumption working women either were single or were supplementing their husband's salary and in either case needed less pay than men. Nevertheless, the essay by Bayes is of fundamental importance both to scholars and to those engaged in public debate on the comparable worth issue.

The essays by Laura Vertz and Keon Chi are also of high quality and make important contributions. Vertz brings together a number of different studies and uses aggregate data to identify patterns of gender-based differences in salaries paid by public employers in different states and to correlate these differences with political subcultures and traditions. Her finding that those states ranking high on indicators of professionalism and good government are the states with the highest gender-based pay disparities is at first surprising, but is then persuasively explained by patterns of job segregation. Chi provides a complementary essay that is rich in detail and understanding both of the various dimensions of comparable worth and of the activities of state governments. It is unfortunate that the editors did not offer observations about how the information and the perspectives of these two essays might be integrated.

The least useful section of this volume is the one on comparable worth and job evaluation. The policy and technical issues that could have been addressed are interesting and important. Instead, the essays are general and superficial and include an almost trivial discussion about how personnel managers can use the concern about comparable worth to enhance their own stature.

The final chapter is inappropriately entitled "Conclusion." It is instead a seven-page summary of the preceding 14 essays. Readers who wish to draw from the case studies and from the variety of theoretical and analytical discussions that comprise the body of the book are on their own. The editors do not provide a rationale for why essays were included in the volume, nor do they share their own views as to what major lessons one might draw from the different contributions. Given the richness of some of the essays, this failure is particularly unfortunate.

DENNIS L. DRESANG

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The Elusive Executive. By Gary King and Lyn Ragsdale. Washington: Congressional Quarterly, 1988. 526p. $22.95 cloth, $17.95 paper.

This book has two missions. The first is to underwrite a movement away from anecdotal-based presidency studies and toward contemporary political science techniques and theories. They cite Hugh Heclo's *A Government of Strangers* (1977), which discommended reliance on anecdotes because, first, it biases research toward writing "trenchant interpretations of current events" and away from providing "in-depth empirical information about what is actually happening in the presidency"; and second, it creates "stylized facts": things "known" through acclamation among presidency scholars, rather than through systematic analyses. As a result, Heclo concluded that presidency research has often accepted unsubstantiated generalizations as fact. By compiling a plethora of real facts, King and Ragsdale support reversing this trend. This volume is a wonderful companion to the American Enterprise Institute's now classic *Vital Statistics on Congress* (1984) and Stanley and Niemi's *Vital Statistics on American Politics* (1988). Like these others, *The Elusive Executive* should be on our bookshelves and in our syllabi.

The second mission is to refocus study away from individual (and often apparently aberrant) personalities and toward what they call the "plural" presidency. This seems to be a call for the kind of "new institutionalism" that is currently popular in Congress studies and has proven productive. (Just for example, see McCubbins and Sullivan's *Congress: Structure and Policy* [1987].) Working in presidential archives the past nine years has made it clear that the presidency is more an institution than a person, and it is time to lend that fact some analytic import. While their intent is admirable, *The Elusive Executive* does little to further this objective.

Some specific successes and failures seem worth mentioning. The publishers tout the book as "the most comprehensive set of data" on the presidency, and it is that. The authors present more than 170 tables on subjects ranging from standards like presidential support to Kessel's data on influential White House aides. (The two *Vital Statistics* books carry 108 and 165 tables. Of course, there are omissions, most notably, data on campaign contributions [via Federal Election Commission]). And some data pose as many problems as they eradicate: Table 2.7 summarizes legislative submissions and indicates that Johnson proposed almost as few civil rights bills (four) in his second term as Nixon (three) did in his first term. Yet obviously, these data do not reveal the nature of the programs (the Johnson proposals included the Voting Rights Act and Open Housing Act), a perfect opportunity for the anecdotal analyst to harp on the inefficacy of such data-driven research. And more standardized coverage in tables is useful. These, however, are nits to pick for the second edition.

The data as a whole, however, are undeniably useful. Without any difficulty, King and Ragsdale suggest several correctives, for instance, Carter has the reputation (i.e., is widely "known") for "coming out fast," inundating Congress with critical proposals. Yet the presidential submissions data make Carter seem far less pushy, almost the norm. The ease with which these correctives float to the surface surely raises the question, What is it exactly that we "know" about the presidency, and in what sense are we using the word *know* when we regularly articulate previously unsubstantiated "facts" as given? Clearly, King and Ragsdale have done a great service to the entire discipline. As junior faculty, Ragsdale should be particularly commended for risking her professional career for the profession's betterment.

The text, however, is less commendable. It proposes an approach and supposedly goes about proving its usefulness. Unfortunately, several chapters begin with a theoretical attitude and identify some variables only to have these merely organize the presentation of data like so many thematic statements. In most areas of political science, such would pass for analysis, but this is not enough by King and Ragsdale's professed standards. Of course, this is holding their toes to a fire only they built. No one else in political science today has such an analysis, regardless of their grand ambitions for presidency studies, and—except for Neustadt, maybe—fewer still have done as much to advance the cause. King and Ragsdale *do* set out a pretty good literature review, for which alone the book could be recommended, and there is an excellent bibliography. This book cannot be ignored because the service it performs is too great and the task it sponsors is the right thing to do.

TERRY SULLIVAN

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The Good Ruler: From Herbert Hoover to Richard Nixon. By Bruce Kuklick. New Brunswick: Rutgers University Press, 1988. 202p. $17.95.

Bruce Kuklick is an historian. This, he explains—not any lack of confidence in the enduring value of his thesis that presidential leadership is successful when it "inculcates a positive temper in the electorate" (p. 29)—is why *The Good Ruler* ends with the presidency of Richard M. Nixon. As for Herbert Hoover and Franklin D. Roosevelt, Kuklick begins with them because their responses to the Great Depression marked the end of the era when "Washington rarely intruded into the life of the ordinary citizen" and the beginning of the modern presidency, an institution in which the "man in the office stood for the nation and the way he was perceived to meet his duties measured the health of the country" (p. 49).

Kuklick's concern is for what makes a modern president successful. Historians and political scientists are mistaken, he argues, when (as is their wont) they equate success with policy achievements. Such achievements cannot be measured, according to Kuklick—How can anyone tell whether other policies (or no policies at all) might not have worked better than those the president pursued? In lieu of measurement, scholars inevitably substitute their own, generally liberal Democratic preferences. Not surprisingly, Roosevelt, Harry S. Truman, John F. Kennedy, and (less unanimously) Lyndon B. Johnson receive high grades; Herbert Hoover, Richard Nixon, and (to a lesser degree) Dwight D. Eisenhower are judged to have been less than successful.

Kuklick proposes popularity with the people as a more measurable, and, given the importance he attributes to "expressive" leadership in the modern presidency, a truer standard for evaluating presidents. "What actually made leaders effective [from 1929 to 1974] was their ability to convey to Americans that the world made sense, that the state had moral authority," he argues. "Successful statesmanship provided hope and the appearance of order that legitimated effort" (p. 28). During the Depression, for example, a president was needed to "assemble the energies of the public even if such assembling led to nothing. It was psychologically important for the people to believe that the state was working for them and against wealthy malefactors, even if the performance achieved nothing beyond itself" (p. 79).

What is involved in meeting the challenge of public leadership? The effort modern presidents make "to represent positively the emotions of the whole people in his own person," Kuklick asserts, involves a "struggle . . . in what I call the political interchange about subversion and countersubversion" (p. 61). Presidents and their political opponents argue about what values are important and about who is the real threat to those values. Sometimes this argument has involved anticommunism, sometimes big business, sometimes rectitude in public life.

By Kuklick's standard, Roosevelt and Kennedy were indeed successful presidents, but so was Eisenhower. Each conveyed what the public valued most at the time he was in office (confidence, reassurance, and boldness, respectively). Truman and Johnson, however, were failures: "Truman helped to make anticommunism a touchstone of political virtue. But he could not control the issue, and neither could Johnson" (pp. 33–34). Hoover failed because, unlike Roosevelt, he did not exude the sort of confidence the public wanted: "FDR's policies may have differed little from Hoover's, but whereas for the people Hoover enunciated them literally with sober eyes downward, Roosevelt's chin invariably titled upward" (p. 45). Nixon posed as a guardian of presidential integrity but undermined that claim in the Watergate affair.

Personality, especially the presence or absence of a sense of self-worth, seems to underly a president's capacity to succeed, in Kuklick's view. Roosevelt, Eisenhower, and Kennedy "believed they deserved to rule and believed they were successful. . . . The people estimated these men at the value they put on themselves." Similarly, the public saw "pessimism and uncertainty in Hoover, inadequacy in Truman, desparate craving in LBJ, and deviousness and insecurity in Nixon" (p. 176). Kuklick believes that neither the mass media nor the modern presidency's capacity to manipulate media image making have distorted the people's perceptions of their presidents' true characters.

The Good Ruler belongs in the familiar category of books that while helping to explain some things, claim to explain everything. Presidential scholars have long believed that a president's standing with the public is part of the explanation for success or failure in the office. They also have understood that symbolic leadership is part of what accounts for a president's standing with the public. Kuklick's book is helpful in understanding symbolic leadership, but the author wants us to accept the notion that such leadership is virtually all of popularity and that popularity is virtually all of presidential success. Students of the presidency would do well to attend to what Kuklick does, not what he says he does.

MICHAEL NELSON

Vanderbilt University

Fortas: The Rise and Ruin of a Supreme Court Justice. By Bruce Allen Murphy. New York: William Morrow, 1988. 717p. $25.00.

In an interview less than a year before his death in 1982, Abe Fortas discussed a topic he had previously avoided: his relationship with Lyndon Johnson. He also gave his interviewer, Bruce Allen Murphy, permission to examine his Supreme Court papers. Murphy has made use of those papers, plus a myriad of others, including materials at the Lyndon Baines Johnson Presidential Library, and the private papers and memoirs of a variety of public figures to recreate Fortas's career. He has interwoven his primary sources with a great deal of secondary material, much of it biographical, to illuminate an array of events—previously unknown, only suspected, or misunderstood—that seemed to conspire to ruin Fortas's career and to have such a profound impact on the Supreme Court that they would change the course of U.S. history.

Murphy insightfully examines the journey that took Fortas from his early life of poverty in Memphis to a life of wealth and power in Washington: playing the violin in the backseat of his 1953 Rolls-Royce, owning a home in Georgetown, a summer house in Westport, Connecticut, a collection of antiques and Chinese art, and three French poodles. He explores the development of Fortas's career from Yale Law School (second in his class in 1933) to the center of the New Deal, working for Jerome Frank in the Agricultural Adjustment Administration, making a strategic move to William O. Douglas in the Securities and Exchange Commission, and subsequently maneuvering his way to the position of undersecretary of the Interior. Murphy portrays Fortas as a man of towering intellect and driving ambition but also with a propensity to make enemies.

At the core of the portrait of Fortas, however, is his dual nature. Murphy skillfully reveals how the contradictions that constituted Fortas's personality explain his ultimate destruction. For example, although he was confident of his legal skills and his ability to solve seemingly insurmountable problems, he also consistently developed a "peculiar sense of paralysis at career turning points" (p. 24). At such times he suffered from physical ailments and debilitating uncertainty, insecurity, and anxiety. In 1946 with the choice of teaching at Yale Law School, continuing in government service, or going into private practice, he finally settled on the last and joined forces with Thurman Arnold only after months of painful indecision. It was that persistent paralysis that made him vulnerable and led him to misstep, Murphy argues.

Indeed, when one considers Fortas's propensity to hesitate at critical junctures in combination with other factors, his downfall becomes comprehensible. One of those factors was Fortas's relationship with Lyndon Johnson. Their association, which began when they met in 1939, solidified when Fortas used his legal skills to keep Johnson's name on the ballot for the Senate election in 1948. Thus, when Johnson became president, Fortas provided advice and assistance on issues ranging from the coup in the Dominican Republic to support for the arts to methods for keeping the plastic bubble over the swimming pool free of fog. Johnson was determined to repay his debt to Fortas by placing him on the Supreme Court. When United Nations ambassador Adlai Stevenson died, Johnson hoped that he could convince John Kenneth Galbraith to accept the position. Galbraith, however, looking for a way to resist Johnson, suggested that Justice Arthur Goldberg might consider such an appointment. The stage was set for Fortas's appointment to the Court. By the time Goldberg decided to leave the Court, Johnson had offered the position to Fortas twice. Although Fortas resisted and was certain he never said *yes*, Johnson ambushed him. Still, Murphy suggests that if Fortas had been more decisive at that critical point, perhaps he could have resisted Johnson and in so doing saved his own career. Another factor in his life that combined with his paralysis at critical times was Fortas's "absolute obsession for having enough money" (Douglass Cater, quoted on p. 196), which may well have influenced his decision to accept the arrangement to receive 20 thousand dollars a year from the industrialist Louis Wolfson, who was later imprisoned for stock manipulations.

Although the standard explanation of Fortas's downfall is that he was simply too greedy, Murphy does not take such a one-dimensional view. While he does not condemn Fortas for his mistakes, he argues that he "created his own vulnerabilities and set himself up for a fall" (p. 592). Some readers, no doubt, will find that Murphy is not sufficiently critical of Fortas. Perhaps the former justice, who urged President Johnson to launch an "all-out air strike" against North Vietnam in 1968 and who bears a great deal of responsibility for the appointment of Warren Burger to the Supreme Court, should be judged more harshly. Nevertheless, Murphy's analysis is valuable because it is not simple; it is filled with the subtle complexities and uncertainties of real life.

Murphy has demonstrated extraordinary skills both in his research and his telling of the story. Indeed, *Fortas* reads like a novel approaching the caliber of Robert Penn Warren's *All the King's Men.* At the same time it is a major contribution to political science. While most of the scholarship in public law falls into the category of either judicial behavior, law, and society or doctrinal analysis, Murphy's work transcends any such classification. That Fortas's work on the Court occupies only one brief chapter seems particularly appropriate for the biography of a member of the Court whose primary concerns and occupations were extrajudicial. The work is important to political science not only for the light it sheds on the off-the-Court activities of the justices but also for the insights it provides into the politics of judicial appointments and the limits of presidential power.

SUE DAVIS

University of Delaware

Plant Closings: International Context and Social Costs. By Carolyn C. and Robert Perrucci and Dena B. & Harry R. Targ. New York: Aldine De Gruyter, 1988. 193p. $38.95 cloth, $15.95 paper.

After a brief analysis of changes in the global economy since World War II that form the context for plant closings, the authors develop a case study of the closing of an RCA television cabinet plant in Monticello, Indiana. They use the case study to shed light on several questions: Why do plants close? How are communities affected by plant closings? How are displaced workers affected by plant closings? What is the effect of plant closings on workers' social relationships and consciousness? What can be done about plant closings and the needs of displaced workers?

The plant, at one time employing twelve hundred workers, closed in December of 1982. The study during the ensuing months included participant observation; monitoring of the local media; statistical comparisons with a nearby, demographically similar county; and formal interviews of workers and their families, plant officials, social service personnel, local politicians and businesspersons. The first stage of the study was a questionnaire distributed eight months after the closing to the displaced workers. The questionnaire appears in the appendix.

The book provides an excellent survey of the literature up to mid-1986 on each of the research questions. The bad news is that this cutoff date, a result of the lengthy publication process, misses several important studies of the effects of plant closings and worker displacement—for instance, Podgursky and Swaim's "Job Displacement and Earnings Loss" and Addison and Portugal's "Effect of Advance Notification of Plant Closings on Unemployment," (both in *Industrial and Labor Relations Review* 41, [1987]), and the Task Force on Economic Adjustment and Worker Dislocation's *Economic Adjustment and Worker Dislocation in a Competitive Society* (1986)—and the maneuvering around the passage of a plant closing bill at the national level. But the good news is that the later studies generally bear out the authors' findings, namely, that the probability of reemployment and of lower reemployment earnings depends greatly on local labor market conditions (p. 68) and the age and gender of the displaced workers. For the displaced RCA workers in a high unemployment region, this meant that 71% of the workers—but 92% of the workers 55 years old and older—were still unemployed eight months after the closing. Another 12% were employed only part-time. Of those who were reemployed either full- or part-time, their 1983 earnings averaged 67% of their RCA earnings for men and 59% for women (pp. 70–75).

The study is valuable, especially from a political scientist's perspective, in its comparison and correlation of the objective conditions of the displaced workers with their perceptions of economic distress, health problems, psychological and social effects, and the causes of the plant closing. Of particular interest is the finding that their confidence in political and business institutions was substantially lower than that of all workers in a national poll and that their confidence in labor unions was substantially higher (p. 106). The study also found that a radical analysis of the causes of the closing was highly correlated with the extent of the economic deprivation caused by the closing (p. 144).

The book provides another bit of evidence on the effects of a plant closing. While the questions initially asked are much more ambitious than the findings reported, every such bit of evidence is important for developing an industrial policy and programs for helping displaced workers. The title is also somewhat of an overstatement, as this is really the study of one plant closing and the "international context" is only a sketchy treatment of global economic conditions.

LAWRENCE E. ROTHSTEIN

University of Rhode Island

The Transformation of Old Age Security: Class and Politics in the American Welfare State. By Jill Quadagno. Chicago: University of Chicago Press, 1988. 253p. $27.50.

For nearly a century, scholars have been wrestling with the problem of "American exceptionalism." Jill Quadagno's *Transformation of Old Age Security* is a major contribution to the literature on old age assistance and offers an innovative explanation for the relative lateness of the development of the welfare state in the United States. Quadagno examines the unique aspects of the rise of the U.S. welfare state by tracing the development of old age provisions from the nineteenth century to the present, situating the specificity of U.S. welfare state development in the context of how welfare states develop generally.

Quadagno rejects a number of explanations of the rise of welfare states by illustrating their explanatory weaknesses. For Quadagno, the logic-of-industrialism thesis fails to explain the timing of various social welfare measures, the social democratic model overstates the impact of the power of the working class on welfare state formation, and some variants of the neo-Marxist theories of the state tend to overstate the view that welfare states are by and large a repressive social control mechanism. As an alternative explanation, Quadagno argues that "the social organization of production determines the nature and form of relief programs. If poor laws are a product of feudalistic agrarian societies requiring an immobilized labor force, the national welfare systems emerge from the conflicts generated by the commodification of labor under industrial capitalism and the need for a mobile labor force" (p. 6). The particular form and timing of welfare state policies have to be located in terms of the political process.

Given this general view of factors shaping welfare state development, what are the unique aspects of the social organization of production and the political process in the United States that gave rise to the specific character and timing of the U.S. welfare state? For Quadagno, to answer this question one must move beyond narrow economistic or politically based forms of explanation. Quadagno argues that to capture the essence of the political process in the United States one must posit three levels of analysis: (1) the level of decision making, entailing an examination of legislative debates; (2) the matrix of power that shapes the decision-making process, entailing an examination of extraparliamentary struggles between classes and class fractions; and (3) the level of politics in which change occurs in the power matrix.

Grounded in rich historical detail and ample empirical evidence gathered from archival and secondary sources, Quadagno traces the complex relationship between the social organization of production and the political process in the United States to separate three factors that shaped the specific character and timing of the U.S. welfare state. First, it was not simply the weakness of U.S. labor that delayed welfare state development but rather the bifurcation within the labor movement between the craft form of unionization and the relative lateness of industrial-based unionization. Moreover, Quadagno argues that the so-called weakness of the U.S. labor movement in the post–World War II period in pressing for an expansion of Social Security is to be located in the structural configuration of class-state relations that led to a depoliticization of mass production unions. Second, the developments in the private welfare sector retarded public welfare programs by providing both mechanisms for social control and mechanisms for the decommodification of labor. Finally, Quadagno argues that the existence well into the twentieth century of two distinct economic formations (the industrial North and the plantation economy of the South) retarded welfare state development as the South was able to translate its economic needs within the political process.

If there are any weaknesses to be found in this book, they certainly do not detract from the main argument. Three points, however, do come to mind. Quadagno does an excellent job in posing the nature of the broad contours of class conflict and how they were reflected within the decision-making process. Yet there is some question as to what distinguished the policy-making process from the policy formulation process. Whereas Quadagno gives us a general sense of the political agenda that set the terrain for decision making, we have little sense of the general policy ideas that set the goals of policy innovation and placed them on the political agenda. For example, is there any way to delineate the class content of reformers' goals? Did they indeed represent a particular class fraction? Also, it seems difficult to me to illustrate the depoliticization of U.S. unions in the post-World War II period without some reference to outright political repression. Moreover, it is unclear whether the South was really a separate economic formation from the North or simply reflected the uneven nature of capitalist development. Although I prefer to view this process as one of uneven and combined development, this relatively minor point would not alter Quadagno's basic argument but perhaps would even enhance it.

The Transformation of Old Age Security is a bold attempt to provide a multilevel analysis of U.S. welfare state development. Jill Quadagno succeeds in providing a unique explanation for the particular development of old age assistance in the United States and the more general understanding of the relationship between the social organization of production and the political process through a specific examination of class and politics in the United States over the past one hundred years. This book provides an exceptionally strong case for the importance of historical evidence for constructing explanations of social transformations and is sure to become a model for explaining various aspects of welfare state development.

RHONDA F. LEVINE

Colgate University

The Politics of the Peace Corps and VISTA. By T. Zane Reeves. Tuscaloosa: University of Alabama Press, 1988. 215p. $20.95.

This political history of two programs that emerged from the activism of the 1960s provides a lens that allows a reader to experience—or reexperience—the turbulence of the past quarter century. Both the Peace Corps and VISTA stand as federal programs attached to broader efforts at policy change: the Peace Corps surrounded by debates over foreign policy directions and VISTA embedded in the politics of the community action programs and other antipoverty efforts.

T. Zane Reeves has written an account of the development of each of these programs, tracing the conflicts and pressures that emerged as a result of dramatic changes in expectations as one moved from the Kennedy-Johnson years to the successive Nixon, Carter, and Reagan administrations. For one who has lived through those political waves, the volume seems almost like a compressed video of glimpses and images of U.S. domestic politics of this era. The author's personal experience with both of these programs hovers at the edge of the volume (and one wishes that he had been more willing to share that experience at both an analytical and descriptive level).

Reeves has used the currently popular concept of "corporate culture" as the analytic centerpiece of the study. Although this framework has some appeal, it does not provide him with an appropriate theoretical vehicle to explain the events he describes. This conceptual "borrowing" from the private sector assumes that organizations have a level of stability—or at least an equilibrium—that is internally defined. The external environment interacts with the organization only at prescribed points, analogous to the private sector market. Reeves seems to assume that the corporate culture is created at the time a program is developed and that this act of creation provides a legitimacy to keep the "political wolves" at bay.

It is difficult to fit this approach into the literatures and frameworks that have emerged from the study of the policy process. Reeves seems to believe that administrative agencies have clearly defined goals that can provide the stability for the development of a "corporate culture." While he admits that Peace Corps did have multiple goals in its enabling legislation, he makes such an acknowledgement as an aside, almost two-thirds of the way through the volume (p. 112). This reader wishes that the author had considered some of the insights that are found in several elements of the policy literature. It is not clear, for example, whether Reeves's two programs moved along a "life cycle." One would not expect the administrators of a middle-aged agency to behave in the same way as those who were a part of the program's creation. Some of the problems discussed by Reeves may have been implicit in the policy design of the two programs. The policy implementation and policy termination literatures might have given the author some additional explanations for the patterns that he describes.

The volume does provoke a series of questions, which, however, it fails to address. What do these two programs tell us about the tradition of voluntarism and voluntary organizations in the United States? Haven't there always been conflicting ideologies subsumed within that tradition? What are the differences between the policy environments of these two programs (interest groups, types of bureaucrats, congressional concerns) that might help explain the different responses to the changing political masters?

At the same time, Reeves's treatment provides an unusual approach to the issues related to conflict between political appointees and career staff. He characterizes the conflict as a struggle between the values found in the organizational culture and the values imposed by successive political appointees. He finds a new vocabulary to use to defend the contribution of career public servants. Even the Peace Corps, designed as a temporary organization with a five-year time limit on headquarter staff tenure, exhibited the same resistance to political change found in other federal agencies. As we emerge from an era of "bash the bureaucrat," this defense is refreshing.

BERYL A. RADIN

University of Southern California

White House Ethics: The History of the Politics of Conflict of Interest Regulation. By Robert N. Roberts. New York: Greenwood Press, 1988. 215p. $37.95.

White House Ethics is a political history (dwelling mostly on the postwar years) of presidential and congressional efforts to regulate conflict of interest in the executive branch. *Political* here has three meanings: First, Roberts deals with the impact of political scandals on the advance and retreat of regulatory reform. Second, he follows the intragovernmental struggle among the White House, Congress, the Justice Department, the Civil Service Commission, various agencies, and ultimately the Office of Government Ethics, over the structuring and implementation of conflict regulations. Here, Roberts traces in detail a series of recurrent debates over substance, enforcement responsibilities, remedies, penalties, and applicability. Third—and this is the book's major chord—Roberts contends that the historical debate over conflict-of-interest regulation has taken place largely along partisan lines.

Republican administrations, Roberts writes, are far more concerned with the efficient management of government than are Democrats; they therefore tend to recruit greater numbers of businessmen for executive office (p. 198). Businessmen, though, are prone to breach conflict-of-interest regulations, partly because of their significant private interests, and partly because of their natural impatience with regulation (p. 171). It is not surprising, then, that "with the exception of Harry Truman, every post-war Democratic president has strongly endorsed tighter conflict of interest regulations. In contrast, few Republican administrations have taken any conflict of interest initiatives" (p. 197).

White House Ethics was clearly meant to be a work of narrative political history; it succeeds admirably in its purpose and certainly fills a void in the recent literature. (The last comparable work, Bayless Manning's *Federal Conflict of Interest Law,* was published in 1964.) Unlike Manning, Roberts does not provide much in the way of explicit conceptual analysis, but interesting theoretical insights do emerge on their own from his historical narrative. For example, there have almost always been laws on the books prohibiting officials or ex-officials from "switching sides," whether they are compensated for so doing or not; that is, laws prohibiting conflict even where there is no interest (pp. 31, 101). On the other hand, there continue to be laws that prohibit officials from receiving pecuniary reward for certain services even when those services are not harmful—or are in fact beneficial—to the public interest; that is, laws that prohibit having private interests even where there is no conflict (p. 160). Thus, Roberts's narrative nicely situates conflict of interest on a spectrum of public concerns ranging from loyalty on the one hand to public office as fiduciary trust on the other.

There is undoubtedly truth to Roberts's conclusion that conflict-of-interest regulation is a partisan issue and that this is so because Republican executive officeholders generally possess and play to more private economic interests than do their Democratic counterparts. Here, though, his narrative provides the makings of insights that had Roberts chosen to make them explicit, would have enriched his conclusion. At times, the debate between Republicans and Democrats appears to have bordered on a kind of inversion of economic reductionism: Republicans, we learn, often actually value an officeholder's having accumulated significant private economic interests if only because those interests are taken to be a reliable sign that the officeholder possesses the real-world knowledge and practical understanding necessary for government service (pp. 58, 198). For Democrats, by contrast, the possession of significant private economic interests is often seen not as evil in itself but rather as a reliable indicator of an underlying and unwelcome "predisposition" or ideological bias (pp. 134–36). Here, Roberts might have cited the discussion in G. Calvin Mackenzie's *Politics of Presidential Appointments* (1981), which at one point quotes Republican Norris Cotton ("If people haven't been successful in private life, they aren't going to be very effective in public service, and anyone who has been successful in private life is likely to have accummulated a little of this world's goods") and Democrat Hubert Humphrey ("You can put all your bonds and stocks . . . in escrow, but don't think you can put your philosophy in escrow") (pp. 102, 104). Roberts's stated conclusion is that the different philosophies of government that the two parties espouse in approaching conflict of interest issues are attributable to their different orientations toward private economic interests. But his narrative supports the intriguing notion that for the parties themselves, the real debate often does not concern the propriety of an officeholder's private economic interests per se but rather the underlying philosophy of government they are thought to signify.

In sum, although Roberts might at times have coaxed a little more analysis out of his narrative, *White House Ethics* is a valuable book. It provides a hitherto unavailable historical account of the politics of conflict-of-interest regulation and will prove to be a rich resource for other scholars.

ANDREW STARK

Office of the Prime Minister, Ottawa

New Directions in Budget Theory. Edited by Irene S. Rubin. Albany: State University of New York Press, 1988. 207p. $34.50 cloth, $10.95 paper.

New Directions in Budget Theory is a collection of eight well-written but somewhat disparate essays on the budget and budget institutions. About half of them, plus Irene S. Rubin's excellent introduction, deal in one way or another with the subject of budget theory. *New Directions* is the right title, for no new budget theory is propounded; and Allen Schick, in a tightly argued piece, questions both the possibility and the utility of a budgetary theory.

The issue of "incrementalism" is a repeated theme, on the whole discarded by the authors as too limited to justify its former acceptance. But Joseph White, who argues for a limited view of the budget process and what it can or should do, believes that defiance of the norms of incrementalism is unlikely to work (p. 196).

The book includes a first-rate piece by Lance LeLoup on the evolution from micro- to macrobudgeting. He concludes that in spite of an appearance of cycles, "the increased orientation towards macrobudgeting is more permanent than temporary" (p. 38).

Naomi Caiden looks at the last 25 years and the rise of superbudgets in an effort to help determine the shape of things to come. She does this in an evenhanded way, contrasting optimistic with pessimistic interpretations of the future, and leaves it to the reader to come to his or her own conclusions.

As noted, Allen Schick drives straight to the point in his inquiry into the possibility of a budgetary theory. His purpose was "to explore the possibility of a descriptive theory derived from the special characteristics of budgeting and expressed in generic terms" (p. 68).

Peter M. Benda and Charles H. Levine present the lessons from two case studies on the conflict between the Office of Management and Budget (OMB) and the Office of Personnel Management (OPM) as to which should control workforce size and which executive development. The analysis argues strongly that the OMB should control workforce size and marginally that OPM should handle executive development. While of considerable merit, the piece does seem somewhat out of place in a work on budget theory. A similar criticism applies to the chapter on community power and municipal budgets by Charles Brecher and Raymond D. Horton. It is about urban power structure and who governs and suggests that analyzing the results of New York City's municipal budget can reveal a good deal about both.

In a more relevant essay, Jeffrey D. Straussman tackles the issue of entitlements and rights-based budgeting. He concludes that rights get in the way of neat and tidy budget theory, which is one reason they have largely been ignored. He warns that budgeting is simply a part of a much larger political and economic system that "cannot be segregated for theoretical convenience" (p. 121). Irene Rubin examines the roles of legislative and authorizing committees, illustrates those roles in three case studies, and sets forth the implications for budget theory. Joseph White, in "What Budgeting Cannot Do," takes us on a historical trip from the origins of the 1974 act through the Reagan Revolution. He asserts that the budget process has failed, in part because Stockman and Reagan tried to force too much through the budget system and confused the budgetary process with the legislative process; the budgetary process was overwhelmed by grand political strategy. His criticisms are clear, thorough, and attractive.

As a budget practitioner I would like to suggest that one might get further toward a theory if the budget were looked on as a struggle rather than a process: "The Constitution . . . is an invitation to struggle" not only over American foreign policy, as Edward S. Corwin has it, but over budget policy as well.

How can it be called a budget process when the key elements of the formal process are largely ignored? The president's budget is dead on arrival. The first budget resolution is routinely passed weeks late. Reconciliation is often not carried out. The taxation committees refuse to be instructed by their respective houses. Tax bills are initiated in the Senate. Jurisdiction over three-quarters of the budget is outside Budget and Appropriations and in the hands of the tax committees. Continuing resolutions are substituted for regular appropriations bills. Rosy scenarios are put forth by the director of OMB and ratified by the Congress to evade the draconian sequester of Gramm-Rudman-Hollings, which, under any honest interpretation of the deficit figures, should have been invoked in each of the four budget years since its inception. The budget is not so much a process as a barroom brawl.

Can the budget not be better explained in the context of divided and shared powers and the invitation to struggle?

HOWARD E. SHUMAN

National War College

Frankin D. Roosevelt's Rhetorical Presidency. By Halford R. Ryan. Westport, CT: Greenwood Press, 1988. 224p. $39.95.

This book is a careful analysis of the words FDR used in front of large audiences. The author is a professor of speech at Washington and Lee University and investigates the composition of Roosevelt's civic utterances, the president's style of address, and his conception of appropriate techniques of delivery. Halford Ryan has made extensive use of the audiovisual material at the Roosevelt Library and brings to bear on this topic all the resources of someone trained in communications.

The result is a thorough and fascinating examination that confirms the sense scholars have had of Roosevelt's mastery of these aspects of U.S. political life. It is, however, disappointing that the author has limited in a traditional way his definition of rhetoric: it is persuasive *speaking*, exclusively. The book thus exclusively focuses on FDR's public speaking, which, Ryan assumes, was intended to motivate the people behind the president's policies. Yet Ryan also notes Aristotle's definition of the art of rhetoric as encompassing the "available means" of persuasion in any given situation. And at various places in the text the author points up different functions of rhetoric —the tapping of public hopes and fears, the unification of an audience, the rehearsing of communal values, and the demonstration of the orator's self-consciousness of his role and station—and their powers and their restrictions.

Consider the 1935 tax act. The Wealth Tax Act promised to "soak the rich" and was one of the most popular measures of the Roosevelt presidency. Nonetheless, it did nothing to alter the structure of income or to produce revenue for the government. (The top bracket applied only once in the first three years of the life of the act—to John D. Rockefeller.) Is it pushing matters to suggest that the activities involved in the passage of this bill were rhetorical?

Or take the 1944 campaign, in which, to dispell rumors of ill health, Roosevelt paraded through Manhattan in an open car for four hours in a driving rain. (It went unreported that the President's car made numerous "pit stops" in garages where FDR was stretched full-length on the floor, rubbed down, and dried off; had his clothes changed; and was given a shot of brandy and put back in the car.) Surely, there is an important sense of rhetoric in which this extraordinarily successful tour fits.

Or look at FDR's declaration of World War II, "rhetoric" that puzzles Ryan greatly. The author acknowledges that the speech did not convince anyone of the need for arms; the Japanese air force had done that. Moreover, that address did not—contrary to what Ryan asserts—justify Roosevelt's previous palavering about international dangers. From 1939 to 1941 the president warned the people (some critics urge he maneuvered them deviously), but Roosevelt's concern was always Germany and aid to Britain and France. War in the Far East surprised the administration and may even have controverted its European emphasis. In any event the attack on Pearl Harbor did not vindicate the president's views about the Nazis. Is it stretching matters to contend that the speech on 8 December 1941 was not rhetoric but an expression of rage against the Japanese that neither persuaded nor attempted to persuade?

The conceptual connection between mere talking and doing is complicated, and so is the causal link between one person's words and the deeds of others. In entirely neglecting even to address these issues, however, Ryan has unnecessarily constricted his enterprise. His study is a detailed and competent account, but it suffers from a narrow idea of rhetoric that makes the text less interesting than it might have been.

BRUCE KUKLICK

University of Pennsylvania

George Washington: The Making of an American Symbol. By Barry Schwartz. New York: Free Press, 1987. 250p. $22.50.

Popular Images of American Presidents. Edited by William C. Spragens. New York: Greenwood, 1988. $95.00.

Barry Schwartz's main thesis is that George Washington "can be seen as the object of a kind of secular religion" (p. 8) indispensable to institution building in the nascent U.S. republic. As seen both by his contemporaries and by culture creators in the early republic, Washington became a symbol of the collective, in the Durkheimian sense. Yet Washington was not a traditional charismatic hero; instead, he was uniquely suited to a republican regime, for instead of attempting to expand his personal power to the detriment of republican institutions, Washington deliberately restrained himself, respected republican institutions, renounced political power in favor of private life, and demonstrated always his awareness of his own personal limitations. The result was a novel phenomenon—democratic charisma—in which heroism became an object not of public awe but of emulation and education.

Because Washington was an accessible symbol of modesty and will power, young citizens could aspire to their hero's virtues and thus become active citizens. Washington's very image of *ordinariness surmounted* induced a habit of political participation among his countrymen rather than passive worship of an authoritarian demigod. Moreover, Washington's oft-avowed preference for private life helped establish an enduring division between private and public, in the United States, thus restraining government growth. Finally, because the celebration of Washington the symbol grew without Washington's personal sponsorship, public institutions could benefit from Washington's popularity without becoming the creatures of Washington the man. The necessary separation of government institution from officeholder could thus be accomplished.

Schwartz's book covers much of the ground that Garry Wills recently traversed in his study of Washington, *Cincinnatus*. Yet, Schwartz's study is more useful to political scientists, because it grounds Washington's formative impact firmly in sociological theory. Schwartz uses Durkheim's sociology of religion to demonstrate that democratic charisma as centered in Washington the symbol uniquely suited the Durkheimian organic solidarity U.S. republicanism demanded. As Durkheim might well have predicted, Washington became the center of a cult, complete with regular birthday celebrations, public assemblies to venerate his deeds, parades, and processions. Most important, revelation became a didactic element of the cult. As Schwartz puts it, "it was insufficient merely to venerate Washington, it was also necessary to know what he believed in—what motivated him—and to reveal this knowledge to others so that they might emulate him" (p. 108). In turn, emulation of Washington's model of public service replenished U.S. republicanism.

The cult of Washington depended not only on the unique character of Washington himself, but also on celebratory culture creators, such as Parson Weems (the influential Washington biographer) and Charles Willson Peale (the Washington portraitist), who turned Washington quite literally into a household icon. Schwartz's imaginative use of Durkheim allows us to see how the hagiographic fusion of the sacred and the secular in Washington as symbol helped move the United States from colony and monarchy to autonomous republic.

Although Schwartz identifies particular conditions that promoted the Washington cult, he overlooks others, such as the inchoate nature of party politics (which gave no organized group an interest in fouling Washington's nest) and the absence of an intrusive mass media that would have revealed more of Washington's warts. Schwartz also falls victim occasionally to a species of mechanical functionalism, in which certain asserted "needs" (e.g., the "need" for unity or for crystallized institutions) call forth appropriate responses in the form of the Washington cult. And Schwartz deemphasizes certain paradoxical qualities of Washington's symbolic legacy. The very pervasiveness of Washingtonia forced those who dissented from the cult into a conspiratorial paranoia that laid the groundwork for later outbreaks of conspiracy theories in U.S. politics. But these are small blemishes in a creative and valuable study.

Less successful is the Spragens volume, a multiauthored compendium of mainly descriptive short chapters on individual U.S. presidents. Spragens describes the work as a "reference book intended to be useful to both scholars and lay students of the presidency" (p. xiv). In this limited guise it may prove useful. Yet the individual chapters are a mixed bag, some (mainly the early ones) describing the actual events of particular presidencies, others (mainly the later ones) focusing on public opinion about, and scholarly evaluations of, individual presidents. But the absence of any unifying theoretical or substantive framework prevents the parts from adding up to a satisfying whole.

No original findings or theories emerge from this work. The final chapter labors towards synthesis; but even here the subject matter is familiar, like the by-now widely accepted cycles of presidential popularity; the recent secular decline in presidential popularity; the appeal of "outsider" presidents. The diligent reader can identify certain patterns the authors themselves do not really underline. The modern image-conscious presidency appears to begin with McKinley, and takes on a form familiar to modern eyes as clearly as Coolidge. Image *manipulation* becomes a preoccupation mainly with FDR and Kennedy, and in Lyndon Johnson we confront a thoroughly postmodern President who troubled himself mightily about the "cut of his clothes" (p. 487) and worried about the "merits of horned rim versus rimless glasses" (p. 488).

The chief paradox in presidential image management is the pervasive gap between effort and success. The final chapter of this volume attributes this gap partly to the media's initial inflation of public expectations about new presidents, followed by media-initiated revelations of presidential failings. Yet the public could hardly have been disillusioned by a Hayes or a Cleveland, not because of a less-powerful media system, but because the men themselves promised little and could therefore deliver pretty much what they offered. More recently, the United States' international role may have tempted presidents to rash promises, with Vietnam only the most conspicuous example. Yet all this matters only if presidential images *themselves* matter, a proposition that is—surprisingly—never actually established by Spragens's contributors. Schwartz makes the cult of Washington not only interesting in itself but central to an understanding of early U.S. political life. Spragens and his contributors ought to have done something at least comparable to justify the price of this quite-expensive volume.

RICHARD M. MERELMAN

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Who Guards the Guardians? Judicial Control of Administration. By Martin Shapiro. Athens: University of Georgia Press, 1988. 195p. $20.00 cloth, $10.00 paper.

"The aim of this book," Martin Shapiro begins modestly, "is to introduce to students of American politics and policy-making a segment of politics that concerns the relationship between administrative agencies and the courts that review them" (p. ix). He is too modest. The book achieves its stated aim; but as the title suggests, it actually addresses the more fundamental issues we inevitably encounter at the intersection of U.S. politics and political philosophy.

Shapiro posits that public policy outcomes and the conventions of policy making that create them fit into clear patterns that shift over time. How, he asks, can we explain the causes of these patterns and their shifts? How, he also asks, ought we to judge the goodness or rightness of policies and the procedures for creating them? Administrative law provides the right arena for examining these empirical and philosophical questions because there, free from the virus of interpretive theory that fevers the brains of constitutional analysts, these questions merge in political practice.

The two merge because Shapiro concludes that patterns in public policy procedures and outcomes (here the policies within administrative law declared by statute and judicial opinion) can only be explained as the product of competing formal theories about the nature of good or right policy. No modern theory or model of effective policy in fact specifies correctly how to tell good policy from bad. Shapiro's own evaluative prescription is thoroughly Grecian, a call for a return to a politics of prudence that rejects the possibility of a science of good policy. However, Shapiro bestows considerable power and influence on modern academic theory builders. The world follows academic prescriptions at the same time it proves them wrong.

Thus, Shapiro argues that pluralist theory led to the judicial expansion of citizen participation in decision making. Pluralist theory's subsequent failure to embrace aspirations for effective community building in turn renewed the philosophical quest for right answers, which the courts transformed into calls for synoptic administrative decision making. But judges, who "are among those who don't know how to do things" (p. 156), cannot today follow the technological complexities of a synoptic administrative record, which is not really a synoptic record anyway. Were agencies to tell the truth, they would admit the impossibility of synoptic accuracy or certainty. Instead, agencies mostly lie (p. 151), and the judicial capacity for meaningful review is therefore jeopardized.

But here Shapiro takes a cautiously optimistic turn. Judges may lose all power to review actual agency decisions (though not ritual agency smoke screens) unless they devise a jurisprudence of prudence:

"For unless the agencies are encouraged by courts to honestly acknowledge the huge role prudence plays, . . . judges will not be able to penetrate what is really going on at all" (p. 169). Since public policy making does conform, with some lag, to currently popular academic models and theories of good politics, Shapiro's optimism follows from his finding that the recent academic emphasis on discourse and the "new pragmatism" can in fact support his "juris-prudence."

Shapiro gets us to this conclusion by several paths, each path enlivened by Shapiro's style, which retains the playful, colloquial and sometimes outrageous mood of the original Russell

Lectures delivered in 1986 at the University of Georgia. Chapter 1, for example, transforms many complex theories and doctrines into sharply edged puzzle pieces that Shapiro deftly moves about in search of his prudential answer to the title question. By page 18 we have met Benthamite utilitarian moral philosophy (p. 3), Paretan optimality (p. 4), polyarchical political pluralism (p. 5), the theory of rent (p. 7), economic theory of regulation (p. 9), postconsequentialist ethics (p. 12), models of synoptic decision making (p. 15), and Kenneth Arrow's voting paradox (p. 16). Chapter 2 gears the development of administrative law since Dicey to these theories, and Chapter 3 then ties to the first two chapters a third track, the development of political ideology concerning technical expertise and judicial power.

Shapiro's verve and clarity will carry beginners a long way, but his student audience will benefit from fairly careful professorial guidance. The book is short on discussion of actual judicial cases (less than a dozen are discussed in any detail), and excellent heuristic examples are sometimes buried in the text many pages after beginners need them. The index will confuse students who want to track down the significance of *Vermont Yankee* or the citation to Colin Diver's "Policymaking Paradigms in Administrative Law." (The index needed another editing after final page proofs.) Shapiro's style alone will not carry the typical student ignorant of the basics of administrative law to this book's end unassisted.

For readers already academically trained in public law, public administration, or public policy I recommend this book heartily. Shapiro has created that all-too-rare commodity, a good academic read in which a fine and familiar mind creates a plausibly coherent grand rendering of a world we may inhabit. Such a work necessarily involves selective simplification and fictionalizing of concepts, data, and experience to bring coherence from chaos. Most academic readers will therefore presumably disagree with at least parts of the analysis. I, for example, know too many cases whose results do not fit the philosophical epoch in which they occurred to accept at face value Shapiro's linkage of academic theory to political practice. And I would have preferred more explicit analysis of the new pragmatism of Richard Rorty and Richard Bernstein. But demonstrated correctness and universal agreement, after all, are not tests of goodness in the prudent, discursive world that Shapiro both preaches and, despite the synoptic yearnings of his own argument, practices.

LIEF H. CARTER

University of Georgia

The Presidential Veto: Touchstone of the American Presidency. By Robert J. Spitzer. Albany: State University of New York Press, 1988. 181p. $23.50 cloth, $10.95 paper.

This book examines the presidential veto power from several perspectives, including its antecedents, its establishment and evolution, its contemporary use, and the proposed item veto. The author's interdependent guiding premises are that there is a linear relation between the growth of the veto power and the rise of the modern presidency, yet that a great deal of similarity exists in the circumstances of veto conflicts over the two centuries of our constitutional government.

Two previous books have addressed the subject in depth. In *The Veto Power: Its Origins, Development, and Function in the Government of the United States* (1890), Edward Mason traces the genesis of the veto power, discusses constitutional and political considerations, and categorizes all vetoes from 1789 to 1889 as affecting the form of government, the distribution of the powers of government, or the exercise of the powers of government. Mason's work remained the only major study of presidential vetoes until Carlton Jackson's *Presidential Vetoes, 1792-1945* (1967). Jackson examines vetoes by individual presidents over two time periods, 1792-1869 and 1870-1945. Though many journal articles have been devoted to the veto power, Spitzer contends that "no modern, comprehensive, analytical work on the veto exists" (p. xvi).

Many methods are adopted to probe the topic. The initial chapter on the creation of the veto employs historical and philosophical approaches to trace the use of the device by the Romans, successive English monarchs, and American colonial and state governors and the debate over its inclusion in the Constitution. Next, the author taps presidential messages to the legislature and congressional floor deliberations among other sources to depict strategy behind, and reactions to, vetoes by presidents from Washington through Franklin Roosevelt. The ways in which the veto contributed to the modern presidency are described. In the examination of the modern veto, previous empirical studies are reviewed together with short descriptions of veto use by presidents from Truman through Reagan and the author's own political and economic analyses. Whereas a public law approach is implemented to assess persisting pocket veto questions, a comparison of federal and state executive-legislative relations is conducted to determine the applicability of the item veto for the presidency.

Of the several areas Spitzer covers, the section on the modern veto—chapter 3—is least appealing. First, in detailing the findings of empirical studies by Lee (1975), Copeland (1983), and Rohde and Simon (1985) he omits a critique of their flaws, which include faulty specification or repetition of independent variables, combining pocket with regular vetoes, and lumping all override actions into attempts or success of congressional reconsideration rather than testing effects of factors at each stage (chamber). Second, the abbreviated discussion of veto utilization by each presidential administration since World War II seems out of balance with the extensive portrayal of nineteenth-century presidents in the previous chapter. Third, the author admits that his inquiry into the fate of veto-threatened bills is "not considered definitive" (p. 103) in that he limits the number of such threats to those being reported by one major newspaper.

The book's concluding chapter returns to the opening proposition. Presidents with personal skills and party and popular support either did not need to invoke the veto or were able to withstand adverse reactions to it, whereas "reliance on the veto by unassertive presidents in noncrisis circumstances may have politically hurt those presidents, but it increased the power of the presidency" (p. 145). In this manner, the veto has more value to otherwise-successful chief executives as a symbolic rather than a substantive power. Scholars disagreeing with the author's observations postulate that personal and institutional characteristics of the office cannot be separated, that many elements have advanced presidential power more effectively than the veto, and that the depiction of strong versus weak presidents conjures up notions of cyclic rather than sequential increases in power.

The author promotes no agenda for future investigation, but does pose a plethora of constitutional questions and ambiguities concerning the veto power. His description of the Tyler, Andrew Johnson, and Ford administrations will no doubt assist scholars seeking generalizations about the political behavior of succession presidents. President Reagan's colorful metaphors on the subject would have us believe that the veto is making a comeback. Using diverse approaches and a wealth of primary research, Spitzer eloquently portrays how it has been a viable yet vulnerable power for presidents all along.

SAMUEL B. HOFF

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Social Regulatory Policy: Moral Controversies in American Politics. Edited by Raymond Tatalovich and Byron W. Daynes. Boulder: Westview, 1988. $40.00 cloth, $16.95 paper.

This volume attempts to place six social issues into a comparative framework for policy analysis. Written by students of Theodore Lowi, the volume follows a common framework to analyze case studies of school prayer, pornography, crime, gun control, affirmative action, and abortion. In addition, Lowi writes an extensive foreword attempting to integrate social issues into his policy typology, and the editors provide a conclusion in which they offer 14 propositions concerning social issues. The volume is useful in its presentation of congressional, presidential, bureaucratic, and judicial action on the six issues. This book maintains a consistent scheme of presentation, and if one is teaching a course on public policy this is certainly a book that merits placing on reserve.

Although the volume is useful as a summary of six policy issues, it fails in its attempt to integrate social issues into Lowi's existing framework and to move beyond the most obvious propositions concerning issue similiarities. The portion of the book that comes closest to offering a comparative framework is Lowi's foreword, where he suggests that social issues can be understood with his policy typology. Social issues are different from economic issues in that they are characterized by "radical" rather than "mainstream" politics. In radical politics those who want social actions regulated want the actions totally eliminated, while in mainstream politics they desire only that the regulated party reduce or channel its actions. Examples of actions to be eliminated would be abortion, discrimination, and pornography while actions to be reduced or channeled would include car speeds, pollution, and utility rates. Although this is a useful distinction, it fails to meet its own criterion of mutual exclusivity. Most abortion control policy suggestions do not eliminate all abortions. Neither the courts nor the legislatures have been able to determine what constitutes pornography or its access to adults. At the same time, many environmentalists want zero discharge of pollutants. The obvious distinction between social issues (radical politics) and economic issues (mainstream politics) is the moral dimension. But the moral dimension is more continuous in nature than dichotomous. When dealing with regulating prescription drugs, rate increases, or pollution discharges, there is an obvious moral component and the extent of this component varies enormously.

In their conclusion Tatalovich and Daynes seek to integrate the volume by drawing propositions concerning social issues from the six case studies. These propositions are relatively obvious and are descriptive rather than analytical. For example, one proposition reads, "Single-issue groups promote absolutist positions on social regulatory policy that polarize the debate as one of nonnegotiable moral alternatives."

Notwithstanding the above reservations, I believe the book is a useful addition to the policy literature. It clearly shows that there are common themes among different social policies and that the political system treats such diverse issues as gun control, abortion, and affirmative action similarly. In addition, the case studies of pornography, affirmative action, and abortion are particularly good summaries of the development of legislation, court decisions, and implementation in these three policies.

Beyond these contributions, the volume makes an implicit advance in policy analysis by arguing that we must rethink past policy typologies to determine if social issues fit existing models. Having done this, I would suggest that social issues fit Lowi's typology better than he believes. Regulatory policies are simply redistributive policies that redistribute rights and duties rather than goods and services. Most social issues supported by the public (i.e., those that wish to support traditional norms such as limiting pornography) attempt to redistribute rights from individuals to society. For example, I would no longer have access to literature that society deems pornographic. It is obvious in such a situation that there is a conflict between individual rights and social norms and, as was the case with the economic policies of the New Deal, the courts will tend to side with their conception of individual or corporate rights while legislatures will side with the majority opinion. But in this respect, social issues are not different from past economic issues. Many of our early economic regulations had the same moral certainty of today's social regulations. Yellow journalism and muckraking polarized issues into moral choices, and the courts and the legislatures responded to those issues as they have to the six issues examined in this volume.

R. KENNETH GODWIN

University of Arizona

Invisible Politics: Black Political Behavior. By Hanes Walton, Jr. Albany: State University of New York Press, 1985. 366p. $44.50 cloth, $16.95 paper.

In his latest book, *Invisible Politics: Black Political Behavior*, Hanes Walton Jr. vigorously and systematically disputes some of the generally accepted social science studies on black political behavior. Walton characterizes the behavioralistic approach as "incomplete, unsystematic, noncumulative, and, in some cases contradictory" (p. xix). The behavioralist research is inaccurate when it consigns blacks to "low levels" of political behavior based on theories of alienation, cynicism, and apathy. In these studies, attention is shifted away from structural and systematic forces in society that limit the political participation of blacks, and the focus is directed toward individual sociopsychological and intrapsychic factors. Furthermore, the behavioralistic approach leads one to conclude that limited black political participation is due to blacks themselves (i.e., lack of political culture). There are few studies that address practices such as registration systems, residency requirements, white primaries, hostile registrars, literacy tests, poll taxes, economic reprisals, gerrymandering, run-off primaries, and a host of other legal and extralegal maneuvers as determinants of black political behavior. In addition, behavioralist research is often limited only to voting behavior. Some neglected areas of study are those that examine black political party behavior, black judicial and legislative behavior, and so on, which provide a more comprehensive picture of black political behavior. As a result of these theoretical and conceptual shortcomings, black political behavior in the contemporary United States becomes invisible; thus, Walton's notion of "invisible politics" is applicable.

In his discussion, Walton also contends that methodological choices made in the study of black political behavior contribute to this invisibility. For example, most surveys either underrepresent or omit blacks from their samples or employ an unrepresentative sample of blacks as a control group. Questionnaires are often limited in scope, focusing on the individual rather than the "complex web of systematic and structural influences" that affect black political behavior. Walton argues that individual factors or characteristics are valid indicators of political behavior only if the interplay between these variables and the political system is examined. Survey data alone does not capture this dynamic. More naturalistic methods, such as case studies and participant observation, which rely on actual observation and in-depth interviewing, offer a multidimensional interpretation of black political behavior (including time and energy expended in participating, cost, resources available, skills, etc.). These methods have the advantage of alerting the researcher to alternative forms of political behavior such as lobbying, testifying, campaigning, and other routine bureaucratic activities.

Walton uses a wide variety of data sources to both critique traditional explanations of black political behavior and expand the notion of what constitutes politics. His own refutations and theoretical reformulations are substantiated with data from a national black population survey, Bureau of Census demographic data, structured interviews, and case studies. A broad range of black political activity is analyzed, including black political party behavior, legislative behavior, judicial behavior, political leadership behavior, and international political behavior.

In summary, *Invisible Politics* fills an important void in the understanding of black political behavior and illuminates some multidimensional and alternative forms of black participation in the political arena. As a result, black political activity is no longer invisible to scholars and policy makers.

JAMES N. UPTON

Ohio State University

When the Marching Stopped: The Politics of Civil Rights Regulatory Agencies. By Hanes Walton, Jr. Albany: State University of New York Press, 1988. 263p. $52.50 cloth, $19.95 paper.

The implementation phase of the policy cycle has been the subject of much debate and the focus of a fair portion of the current research in public policy. A major criticism of implementation research has been that it has taught us little about how to improve public policy and even less about how policy is made. Moreover, the proverbial use of the single-agency case study approach has precluded, some suggest, the development of a generalizable theory about implementation. (There is disagreement, however, as to whether a generalized theory of implementation is possible or really needed). Walton's book represents a refreshing and new approach to implementation research. It is a comprehensive and detailed analysis of the implementation of Title VI (discrimination in programs receiving federal funds) and Title VII (employment discrimination) of the Civil Rights Act of 1964 across virtually all of the statutory and executive agencies of the federal government from 1964 to 1982.

After reviewing the implementation literature, Walton was unable to find a suitable framework within which to posit his analysis. He concluded—correctly—that the environment, actors, and political relationships surrounding the implementation of civil rights policies differed substantively from the conditions and policies examined in the extant literature. (Nor was Bullock and Lamb's *Implementation of Civil Rights Policy* [1984], although similar in topic, useful for Walton's purpose.) The fact that Title VI created civil rights administrative units within each federal agency and department and Title VII created the Equal Employment Opportunity Commission necessitated the development of a framework that would examine implementation across agencies.

This well-documented book, including material obtained under the Freedom of Information Act, covers a number of important aspects of the implementation process. One area is the political bargaining surrounding the creation of the Title VI offices within each department, as well as the Equal Employment Opportunity Commission. In spite of the passage of the Act in 1964, "it took eight years before all of the federal departments at cabinet level had created units to carry out the Title VI congressional mandate" (p. 35). The agencies varied in the structure of their compliance mechanisms. Some units created separate offices of civil rights compliance, while others placed the responsibility with equal opportunity divisions. Moreover, directors of these units did not have lengthy tenures. Walton concludes that "the regulatory efforts of these civil rights agencies have been significantly influenced by when they were created, how they were structured, and the degree of stability in their top administrations as well as the functions assigned to their full-time permanent staff members" (p. 55).

He continues by examining the impact of budget allocations for Title VI activities across agencies on the enforcement process. The results indicate that "amongst the four presidents—Nixon, Ford, Carter and Reagan—the funding of Title VI enforcements, with the exception of one year, 1979 in the Carter administration, has not been a major political priority" (p. 68). In regard to actual enforcement of these titles, Walton suggests that "political pressures and a variety of internal governmental factors have seriously eroded effective enforcement of Title VI rules. Federally funded, illegal discrimination has continued largely unabated" (p. 121).

The impact of pressure groups, pro and con, on the enforcement of Title VI and Title VII is also explored. Civil rights, strange as it may seem, has now been defined as a "special interest," which, Walton concludes, has resulted in an anti–civil rights attack by neoconservatives, first, white, then black. This alliance proved to be a very effective pressure group and "pressure group activity at least in the two Reagan terms, had the overall impact of reducing the federal government commitment to civil rights regulatory enforcement" (p. 175).

Paradoxically, the main strength of the book—extensive and unique documentation—also proves to be its principal weakness. The book is so thoroughly researched that it bogs down in places and does not flow as smoothly and freely as it should. I found myself drowning in the information, not sure just how to synthesize all of the data presented. The latter part of the book moves more quickly, allowing for the synthesis that did not occur in the middle.

For all the uniqueness of its approach to implementation, Walton essentially takes a "top down" approach to implementation. This is the classic approach to implementation research and has been criticized as limiting our understanding of the implementation process. Much of the recent literature has suggested alternative approaches that provide a more enlightened and informed view of the process. Nevertheless, Walton's book is a major contribution to our understanding of the implementation process in general and implementation of civil rights policy in particular. It will clearly have an impact beyond Walton's original intent.

PAULA D. MCCLAIN

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The Politics of Social Policy in the United States. Edited by Margaret Weir, Ann Shola Orloff, and Theda Skocpol. Princeton: Princeton University Press, 1988. 465p. $52.50 cloth, $14.50 paper.

When Theda Skocpol burst on the academic scene with the publication of *States and Social Revolutions* (1988) and an agenda of "Bringing the State Back In," she made a major contribution. She enjoined us to pay attention to a near-forgotten European tradition emphasizing the state as a formidable judicial, military, and extractive apparatus. As this agenda is actually developed by Skocpol and her colleagues in their work on U.S. social policy, however, the state means something less.

This book is based on papers presented by scholars at a series of conferences at the University of Chicago in 1984-85 organized by Theda Skocpol and a number of her students—who authored about half of the papers and two of whom joined her in editing this collection. This review is thus an opportunity to assess the "state-centered" approach to the interpretation of U.S. social policy (although I should note that the papers prepared by scholars outside Skocpol's group do not adopt the state-centered perspective, the editors' rather imperial claims notwithstanding).

The "institutional arrangements of the state" consist of some aspects of the electoral-representative system and some aspects of government structure understood as bureaucratic capacities and the constraining influence of existing policies and their constituencies. What we have here is a revival less of the grand state conceived by Weber and Hintze than of political science circa 1950, with its singular focus on the internal organization of government, a kind of public administration approach to politics writ large. This is actually not a bad thing. It is useful to be reminded again of the importance of the distinctive organizational environment of state decision makers. But if we are not to swerve mindlessly from one explanatory fad to another, the question is not whether the organization of government matters; it is rather how these "institutional political" variables weigh compared with other explanations.

Although the editors promise such a synthesis—politics and policy, they say, are "jointly conditioned" by the organization of the state and by class and other social relationships—the historical accounts of U.S. public policy emphasize little except governmental arrangements. Other factors are mentioned only in passing. Orloff discusses the role of middle-class and professional strata in opposing social spending during the Progressive era, for example. Weir, writing of the development of unemployment policy during the Great Depression, notes that there was "profound disruption" and mounting demands for governmental action. Finegold tells something about the political complexities of the South in his discussion of agricultural policy. Still, it is to my mind something of a feat to compile a history of the development of U.S. social policy from which popular movements of the unemployed, the aged, industrial workers, blacks, and women have been virtually deleted except as anecdotal asides and in which business figures not at all, having been conveniently buried in a variety of "expert" and "reform" networks whose existence fits rather better with the state-centered argument. "Political processes other than the initiative of capitalists," the editors can then announce, "have nearly always been the cause of U.S. social policy innovations" (p. 14). Even in the current period, business mobilization against social spending hardly emerges as a topic for analysis.

One theory cannot do everything, and the "institutional-political" realm is sufficiently significant that clarification of the role it played in shaping the evolution of social policy in the United States would be welcome. But the accounts presented here use state-centered variables episodically and erratically. There is no attempt at an overall delineation of the relevant features of the "institutional political" realm, no attempt to concretize those features, and no systematic exploration of how they operated to shape a given policy. Instead the authors pick and choose features that suit their purpose—here a bureaucracy, there a policy expert network, and so on. As long as some such institutional-political factor can be found in the story, it constitutes fuel for the state-centered engine. (Meanwhile, no one in this volume even touches on the rising power of the military bureaucracy and its impact on social spending.)

There is also the question of just what counts as evidence of the weight of institutional-political variables. There are always bureaucracies about, but when are they influential and why are they influential? Is it reasonable, for example, to attribute the decentralization of key social policies to the states during the New Deal to the prior existence of a few puny state old age or unemployment programs? Or is it reasonable to argue that the Great Society programs generated conflict because they were "fragmented" (i.e., oriented specifically to black constituencies) without looking at what happened to programs that were not similarly fragmented? Universalizing efforts to integrate public housing or schools had in fact provoked furious battles. The creation of "fragmented" programs in the Great Society era that would reach blacks without intruding on white prerogatives was one way of coping with race conflict. In general, U.S. government is not fragmented and decentralized by accident. Governmental organization matters, but it is itself rooted in a larger politics, whether racial politics or the conflictual politics generated by sharply different patterns of regional economic development.

The argument that policy fragmentation leads to fragmented political support is made most stridently with regard to the Social Security Act. Because the act created two streams of policies—universalistic old age pensions and unemployment benefits that reached a majority of citizens and "means-tested" public assistance programs that ended up with a disproportionately black constituency—the means-tested programs are presumably more vulnerable politically. (The ostensible political virtue of universalistic programs has long been an article of faith among social welfare professionals.) But inconvenient facts are ignored. For example, unemployment insurance benefits are not better than Aid for Families with Dependent Children (AFDC) benefits—the level of support is much less than AFDC when food stamps and medicaid are factored in; unemployment insurance eligibility terminates after six months, while AFDC eligibility can run on for years; and although the AFDC program suffered cuts during the Reagan years, the unemployment insurance program was literally butchered as part of a larger effort by the Republicans and business to increase labor discipline and hold the line on wages. In a word, the relations between politics and public policy are far more complicated than the state-centered advocates are asking us to believe.

The editors conclude that a not-unfamiliar list of policy prescriptions and a call for "a new imaginativeness" by intellectuals and politicians as a major step toward implementing them. More likely, just making up for recent losses will entail bitter conflict, and by just the groups the state-centered school ignores.

FRANCES FOX PIVEN

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The New Politics of the Budgetary Process. By Aaron Wildavsky. Glenview, IL: Scott, Foresman, 1988. 468p. $16.60 paper.

The New Politics of the Budgetary Process is not a fifth edition of Aaron Wildavsky's classic treatise on federal budgeting. Of course, the style remains true to the original. Those who found themselves occasionally cringing from his punchy declarations will not find total relief in this new and improved treatment. Some vintage Wildavsky has been updated to take account of current budget controversies. Here, for example, is Wildavsky on entitlements: "Only the naive think that political power plays no role in entitlements; only the foolish think that politicians' perceptions of who is deserving play no role in determining who receives entitlements" (p. 286). But there are fewer uncompromising assertions of this sort. The major alterations in federal budgeting since 1974 and the corresponding complexity of the process call for more detail, more analysis and, yes, even some subtle hedging compared with the four editions of Wildavsky's classic. Scholars will therefore find *The New Politics of the Budgetary Process* a good way to learn how our most-quoted interpreter of the federal budget process has shifted his central argument concerning incrementalism. Undergraduates and even some graduate students will find the book quite challenging if they are completely unfamiliar with the central features of contemporary budgeting.

The most familiar part of the four editions of *The Politics of the Budgetary Process* is retained as chapter 3. Incrementalism—the various methods of calculation and the strategies employed by the major participants in the process—are repeated with the old vignettes. Who can forget the poor State Department official who tried to convince an extremely skeptical Congressman Rooney that sending a Chinese-speaking foreign service officer to London so that he could practice his language skills while eating lunch in a Chinese restaurant in Soho with his British foreign office counterpart was an appropriate use of the taxpayers' money? The chapter will still make you smile. But Wildavsky titles the chapter "Classical Budgeting," suggesting a bygone era.

What has changed? Most of the book is devoted to the theme of the loss of consensus—the erosion of basic rules of the budgetary game. Wildavsky pursues this theme by analyzing some of the more important changes that have occured in the budget process since the first edition of his now-classic appeared in 1964. He covers entitlement programs in detail. Federal credit activities are discussed. Structural changes brought about by both the Budget and Impoundment Control Act of 1974 and, later, the Balanced Budget and Deficit Reduction Act of 1985 (more commonly known as Gramm-Rudman-Hollings) are also given full treatment. Defense budgeting receives its own chapter.

Readers may quibble with some of Wildavsky's interpretations of these changes in federal budgeting. Yet it is the final chapter that will produce a new round of Wildavsky-induced controversy over the theoretical significance of the "new" politics of budgeting. What, after all, replaces the past norms of budget behavior like *balance* between taxing and spending; *comprehensiveness* of budgetary accounting; and *"annualarity,"* which provided a modicum of predictability in an otherwise unruly process? Wildavsky reviews some of the palliatives so often repeated such as a constitutional amendment to require a balanced budget, a presidential line-item veto, a separate capital budget, and multiyear budgeting. He is not sanguine about the long run impact of any of them, a view that may disappoint some who still look for a budgetary quick fix. The resolution? *The New Politics of the Budgetary Process* ends on an incomplete note. Wildavsky concludes, "As much as I respect the importance of budgeting and the talent of budgeters, to substitute budgeting for governing will not work" (p. 439). The diagnosis of the "governance crisis"—a taller task than the elusive search for budget theory—must be found elsewhere.

JEFFREY D. STRAUSSMAN

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Markets or Governments: Choosing between Imperfect Alternatives. By Charles Wolf, Jr. Cambridge: MIT Press, 1988. 220p. $18.95.

The theory of market failure is a staple in every public policy course. Economists have developed a very sophisticated analysis of the market's inadequacies: of spillovers and externalities, of public goods and monopolies, of imperfect information and inequities. In this provocative book, Charles Wolf, Jr. argues that nonmarket institutions—especially government—also have characteristic failures that flaw their performance. Developing a theory to explain such failures is the goal of this book.

Without a bottom line, Wolf writes, governments find it difficult to restrain the expansion of programs. He also contends that elected officials' short time horizons lead them to pursue immediate benefits without always fully weighing long-term costs. Thus, governmental decision making tends to be myopic and inefficient.

Moreover, nonmarkets often "decouple" burdens and benefits. Those who benefit from government programs frequently do not have to pay their full cost. Wolf extends this classic free-rider problem to explain why government sometimes does not act. Weak gun control produces substantial but dispersed problems for society, while the costs of complying with stricter gun control would be concentrated on organized groups, especially the National Rifle Association.

Decoupling also helps explain the growth of governmental programs. In what he labels "microdecoupling," benefits are concentrated, but the costs are dispersed. Thus, in programs ranging from agricultural price supports to social security, recipients can effectively demand higher benefits to be funded by a broad, less-organized group of taxpayers. "Macrodecoupling" leads, in reverse, to exploitation of the minority by the majority. The demands of lower-income individuals for taxes on those holding wealth, he contends, can erode economic investment and growth.

Wolf merges these notions to create a theory of nonmarket failure. First, nonmarkets tend to have a disjunction between costs and revenues, especially because of decoupling; this disjunction produces redundant programs and rising costs. Second, nonmarkets are plagued with what Wolf calls "internalities," bureaucratic motives that serve narrow, not public, purposes. Third, nonmarkets suffer from "derived externalities," or unanticipated consequences. Finally, nonmarkets are troubled by distributional inequities, with power and privilege vested in the hands of some, but not all.

Markets are often more efficient than nonmarkets, Wolf concludes, but he also recognizes that in public programs other criteria, such as accountability and representation, are important as well. Rather than trying to resolve the dispute, he concludes that nonmarkets can remedy market failures, and vice-versa. Since one system is the antidote for the other, the key, as he expresses it in the book's subtitle, is "choosing between imperfect alternatives."

The book makes a strong contribution in arraying the characteristic problems of government compares with private sector activities. Unlike much public choice theory and its privatization cousins, Wolf's book is not saddled with ideological baggage that colors his conclusions. The first half of the book, in which he compares the failures of public and private organizations, is a rich contribution both to economic and to organizational theory.

The book is shakier, however, when it moves on to prescription. Wolf contends that his argument can help illuminate public policy analysis. He is right, but he only barely sketches out how; and in doing so he fails to draw important links with the rich literature on implementation that has developed over the last 20 years.

Moreover, Wolf's plea to watch for "internalities" and "derived externalities" failed to alert him to the dangers of his own prescriptions. To make defense contracting more efficient, he writes, "one might try to inject competitive discipline into a market that involves only a single procurement source" (p. 166). When the Pentagon did precisely that, demands by defense contractors for inside information led to abuse by consultants and a major scandal in the last months of the Reagan administration.

This case represents more than just Wolf's failure to anticipate the unanticipated consequences of previous procurement reforms. It illustrates a fundamental flaw in the book's argument. To a large and growing degree, the boundaries between government and the market are disappearing, and the public and private sectors are becoming inextricably linked. The problem is not one of "choosing between imperfect alternatives" but of effectively meshing government and the market. The risk, as the defense procurement scandal illustrates, is that such a merged system will stimulate the pathologies of both systems rather than cure the problems of either.

The failure to deal with the reinforcing failures of such mixed systems is a singular flaw in the book. Nevertheless, the development of a theory of nonmarket failure not tinged with the ideology of the privatization movement is an important theoretical contribution that unquestionably enriches the debate over the organization of social action.

DONALD F. KETTL

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COMPARATIVE POLITICS

Political Survival: Politicians and Public Policy in Latin America. By Barry Ames. Berkeley: University of California Press, 1987. 286p. $38.00.

Political Survival: Politicians and Public Policy in Latin America by Barry Ames is a significant contribution to the study of comparative politics in general and Latin American politics in particular. Ames convincingly demonstrates the indispensability of strategic coalition analysis as a complement to the "grand theories" of class theory, dependency theory, rational choice political economy theory, bureaucratic-authoritarian theory or any other popular theory in comparative politics.

The public sector is defined as the sum of general government and autonomous state-owned enterprises. Furthermore, general government consists of central government plus decentralized government agencies such as municipalities, state and provincial government, social security institutions and all other noncentral government agencies that are not government enterprises or public corporations.

Ames examines political survival primarily in terms of central government expenditures. Part 1 provides a Baedeker's guide to Latin American politics. Chapter 1 explores budgetary politics in 17 Latin American countries between 1947 and 1982 by examining annual central government expenditures. Chapter 2 examines the relationship between survival strategies and budgetary trade-offs. And chapter 3 examines the policies of administrations surviving their immediate crises.

Central government expenditures, like all public sector expenditures, involve, on the one hand, the producers of collective or semipublic commodities (services) and, on the other hand, the consumers of these commodities. In the analysis of survival politics by Ames it is not clear whether Latin American leaders, in their efforts to hold onto their jobs, have used governmental pork to influence those *providing* the composite commodities (e.g., the military; the teaching establishment; the bureaucrats; the agricultural, public works, and health service producers) or those *using* them (e.g., the sick; the students; the rich, poor, or middle-class consuming public) or both.

This brings me to a broader, fundamental issue, which, in many respects, is at the core of the theory of survival politics. Pork—in particular central government expenditures—reflects the benefit, the ophelimistic side of such politics. However, pork, in the sense of a free commodity without a cost of production, does not exist. Public sector expenditures have to be financed through taxes, loans, fees, grants, and so forth. Unless voters or interest groups are irrational or suffer from illusions, they look at "net pork," that is, the net benefits from government actions, the difference between expenditures and the cost of covering them.

Ames almost totally ignores the cost side of the politics of pork. At the empirical level this is only partly understandable. Tax statistics for the countries and periods Ames has examined are as good as the expenditure statistics he relies on. At the theoretical level, a theory of survival coalitions that considers only one side of the coin, namely expenditure, but ignores the cost side cannot be considered general. Formulating and testing a general theory of survival coalitions is a formidable task. However, in its present partial formulation and testing, the theory of survival coalitions leaves itself open to the criticism of incompleteness and partiality.

Furthermore, the politics of pork includes not only central government expenditures (centralized pork), which Ames examines thoroughly, and tax and other government revenues (negative pork or costs of pork), which are not analyzed, but also the "noncentral" part of general government expenditures ("noncentral" general government pork) and, equally important, the expenditures of state-owned enterprises (decentralized pork), which Ames does not examine in part 1. The missing block of decentralized pork is too important to be ignored.

Still, the comprehensive and convincing statistical tests of part 1 make a significant contribution by demonstrating the complexity of the relationship between central government expenditures and politics.

Part 2 applies strategic coalition theory to Brazil. Both chapter 4, which examines the role of the Brazilian Congress in budget making between 1947 and 1964, and chapter 5, which treats the policy consequences of the transition to democracy of Brazil's post-1964 military regime, contain a superb examination of Brazil's political history between 1947 and 1984. Indeed, in part 2, Ames adds missing building blocks by dealing with such noncentral government pork as rural credit, land reform, housing, industrial deconcentration, and wage policy.

In summary, this is a compact, lucidly written, rich-in-observations study of survival politics in Latin America. Its thorough statistical testing of a wide range of hypothesis and pragmatic approach are most welcome. The theory of survival coalitions advanced here, though still embryonic, is breaking significant new grounds. Political scientists as well as historians, economists, and other Latin Americanists will greatly benefit from this first-rate monograph.

MARKOS MAMALAKIS

University of Wisconsin, Milwaukee

Rethinking Development: Modernization, Dependency, and Postmodern Politics. By David E. Apter. Beverly Hills: Sage, 1987. 328p. $29.95 cloth, $14.95 paper.

The study of political development is, in the minds of many political scientists, moribund. The theory construction projects of the 1960s have been abandoned. The debate between the modernization and dependency schools has waned. Many scholars have returned to area studies, being content merely to describe development within countries rather than to generalize across countries. Apter's book, like several others that have recently appeared, is an attempt to revitalize this field. It is composed of excerpt from books and reprints of articles Apter has written over the last 25 years, and a new essay entitled, "Rethinking Development." In this essay Apter proposes to combine elements of the modernization and dependency perspectives and to add new "theoretical elements" derived from critical theory. In this context, he urges students of development to move from scientific to discourse analysis in search of the "mytho-logics" that shape and reflect our interpretations of development. This new approach will produce a deeper understanding of the theoretical problems that Apter has been preoccupied with over the years, including the problem that constitutes the postmodern problematique: the predisposition for violence against the state.

The book makes a number of contributions. To begin with, it shows how Apter's work has evolved over the years, from the classification and analysis of the workings of different political systems to the study of how groups and individuals interpret development processes. Thus, the chapters in the first part of the book advance highly abstract, cross-national generalizations about development, while the chapters in the second part of the book describe the discourse and beliefs of protest and terrorist groups. In this sense, the book charts the development of the field. Second, the latter chapters are well organized and useful. They reveal some interesting facts about protest movements and terrorist organizations. These essays are suggestive of certain problems or common crises that postmodern societies face. One of these is the problem of pluralism and governmental overload. Apter's work, like that of others, charts the growth of interest groups and interest group demands on modern government; and it illuminates the difficulty democratic institutions increasingly have in coping with these demands. What makes Apter's analysis significant is that he places this problem in a developmental context, showing how and why the overload problem is so severe in modernizing and postmodern societies. Finally, Apter's new work on "mytho-logics" of development promises to contribute to our understanding of the motivation and behavior of groups and elites. It intersects with and informs the new work on the impact of ideas on development policy, for example.

The book has several weaknesses. There are no meaningful transitions between some of the chapters. In fact, the relationship between the first and second part of the book is unclear. For instance, the chapters on violence and terrorism are not presented as tests of the abstract propositions in the first part of the book. Rather, they are related to chapters 2–6 merely by a focus on some common problems (p. 10); the implication of Apter's new essay is that each chapter simply tells part of or an episode in the story of modernization.

As it turns out, storytelling is not far from Apter's notion of methodology. He advocates a move away from behavioralism and operationalism and toward the study of discourse, that is, myth creation and destruction. The rationale for this new approach is never adequately developed. Apter asserts the inadequacy of behavioral methods more than he demonstrates it. He offers no concrete guidelines for discerning and analyzing "mytho-logics" beyond doing field research or conducting interviews. Students of development would have benefited from learning more about the difficulties Apter had in attempting to use behavioral methods to falsify some of the theoretical propositions he advanced in his early works (chaps. 2–6) and about what interview techniques he used and uses in his field research.

The rationale for Apter's new theoretical synthesis also is underdeveloped. The review of modernization and dependency theories in chapter 1 is unsatisfying. It attacks rarefied versions of the two perspectives, if not theoretical straw men. For instance, there is no evaluation of the new Transition from Authoritarian Rule project or of the new research on the impact of ideas on the one hand or of dependency theorists' recent reflections on the fate of bureaucratic-authoritarian states on the other. These and other new works recognize and at least partly answer many of Apter's criticisms. Nor is there any critical evaluation of the liberal theories and behavioral epistemologies that continue to dominate the thinking of the "technocratic elite" who operate the World Bank and other development institutions. A more current and concrete review of these works and epistemologies would have led to an examination of "mytho-logics" that are more squarely at the heart of the debate between modernization and dependency theorists, such as the so-called "Brazilian miracle." Specifically, Apter's weak review of existing theories and methods greatly diminishes the force of his claim that we have bound ourselves to "mytho-logics" about development (p. 321), and it weakens his case for adapting his new approach to this subject.

In sum, this book is worth reading both as a story about the development field and as a source of ideas about how to uncover and study stories about development within modern societies. For people who, like myself, still believe in some notion of a science of development, the first part of the book advances some interesting generalizations about the determinants of social protest and other matters, generalizations that may be worth testing and refining. Beyond this, the book will be at best a call for making a general theory of development still more general (and hence nonfalsifiable) and at worst a call for more description of development processes.

JOHN R. FREEMAN

University of Minnesota

Economic Reform and Political Change in Eastern Europe: A Comparison of the Czechoslovak and Hungarian Experience. By Judy Batt. New York: St. Martin's, 1988. 353p. $49.95.

Current developments in the Soviet Union, China, and some East European countries have drawn attention once again to the question of linkages between economic and political reforms. Some Party leaders have tried to implement economic reforms with only limited political changes, while others—Mikhail Gorbachev and Deng Hsiao-ping, most notably—have maintained that only with political reforms can economic ones succeed.

Conventional wisdom holds that in 1968 the Czechoslovaks argued the case for linkage between economic and political reforms, while the Hungarians maintained that significant economic reform could be achieved with no more than cosmetic and symbolic political changes. The invasion of Czechoslovakia by her socialist neighbors and the successful, if uneven, reform of the Hungarian economy seemed to settle the argument, at least on the pragmatic level.

Judy Batt's thoughtful analysis argues that economic reform did not create functional requirements for political reform either in Czechoslovakia or in Hungary. Historical and situational differences between the two countries explain why political reform was attempted in Czechoslovakia and not in Hungary. She cites the unity of the Kadar leadership compared with disarray in the Czechoslovak elite, as well as the deliberate stifling of political discussion in Hungary compared with the politicization of economic reform in Czechoslovakia resulting from the alienation of the intelligentsia there. Presumably, an equally alienated Hungarian intelligentsia was prevented from raising the specter of political reform by a united leadership living in the shadow of 1956.

According to Batt, in Czechoslovakia economic developments and political crisis ran in parallel, and the only link between the two was the reformist intellectuals. Failure of economic reform led economic experts—who were not highly competent in any case—to advocate political changes. In Hungary, politics remained an elite game and the population remained inward-looking, individualistic, and uninvolved in the policy process. Alexander Dubcek had the problem of "containing a massive impetus to participate" (p. 289), whereas Janos Kadar's was to inject life into a population that saw no value in political activity.

Batt goes over familiar ground in her thorough description of reforms in the two countries. Her main contribution is to show, with commendable lucidity and parsimony, why there were different outcomes to economic and political reform efforts. She is also to be commended for placing the story in the larger context of debates about socialism and the relationship between economic and political systems. Her discussion of Von Mises, Mayek, Lange, and Brus is excellent. One point worth noting is her warning that while market-type economic reforms legitimate group and individual interests, this does not necessarily lead to political democracy. Those rushing to embrace the notion that current reform attempts are making the USSR into a "civil society" should note Batt's warning that one cannot democratize "simply by moving along the quantitative axis of pluralization" (p. 44). Marketization of the economy, she says, is quite compatible with one-party rule and does not imply democratic revolution—or even evolution.

ZVI GITELMAN

University of Michigan

The Rise of Egyptian Communism, 1939–1970. By Selma Botman. New York: Syracuse University Press, 1988. 212p. $29.00.

This book is an exploratory study of the "rise and experience of communism within Egyptian society" (p. ix). It represents an effort to explore the history of "an extraparliamentary opposition which functioned on the periphery of Egyptian politics and society" (p. 150). In spite of this marginal role and status, however, Botman maintains that the communists in Egypt are rightly worth studying for a number of tangible reasons.

The main reason motivating Botman's study is that this communist movement's "history is virtually unknown" (p. ix). This is only true, of course, for the non-Arabic-speaker. There is certainly a wealth of information, documentation, and scholarship available in Arabic on the Egyptian communist movement. Chief among them are the 10-volume works of the distinguished Egyptian historian Rif'at el-Said. While Botman's bibliography cites some of the Arabic literature available on the subject, rather than culling this literature to explore the "rise and experience" of the movement, her methodology basically relies on interviews of a sample methodologically unspecified. Why she undertook the expensive and time-consuming interview method to compile information readily available through published Arabic sources remains unclear. Meanwhile, Botman does not provide an explicit discussion of the theoretical, conceptual, or methodological foundations of her study. It appears, in fact, that there was no explicit, systematic framework; and this gives the entire volume a casual journalistic tendency.

What unfolds, then, is a chronological description of events and post hoc reflections on them by participants or observers or both.

In chapter 1, the author overviews the general milieu of Egyptian society during the 1920s and 1930s. Chapter 2 basically attempts to profile the social class background of Egyptian communists. Chapter 3 then surveys the historical evolution of the plethora of Egyptian communist groupings between 1942 and 1947, while chapter 4 traces the dynamics of consolidation of these loosely held groups into one unified organization between 1947 and 1952. Chapter 5 examines the relationship between the communist movement and the new military junta in Cairo. The final chapter provides the standard summary and conclusions, as well as bringing the study up to 1970.

The time frame of the chronology is itself somewhat puzzling. The dates seem to be arbitrarily chosen; for as Botman reveals, the movement antedates World War I and was disbanded in 1964, when activities effectively ceased. The remaining period barely justifies a postscript yet it is prominently mentioned in the book's title.

In the final analysis, Botman's study is perhaps interesting to the nonspecialist. It is not a study in the scholarly sense—deliberate, purposeful, or systematic. Thus, the issues of validity and reliability that are the basis of scholarly evaluation may not be the appropriate evaluative criteria in this case. Moreover, what may well be more representative of Botman's work is probably her original Ph.D. thesis, "Oppositional Politics in Egypt, 1936–1954." The current title of her book, a revision of her original thesis, is somewhat misleading.

T. Y. ISMAEL

University of Calgary

Political Participation in Rural China. By John P. Burns. Berkeley: University of California Press, 1988. 283p. $35.00.

John Burns's book is yet another nail in the coffin of the discredited Maoist myth of revolutionary socialist democracy. Supplementing official Chinese sources and secondary accounts with refugee interviews, Burns describes a peasantry that fails to play the active role ("masters of their own house") assigned it by the Chinese Communist Party (CCP). Indeed, despite adopting a generous definition of political participation (almost any activity by private citizens designed to influence the formulation or implementation of political decisions), behavior of this sort is remarkably rare in Burns's study of rural China between 1962 and 1984. Rarer still is the occasion on which it is effective. The unavoidable conclusion is that Chinese peasants have been subjects, more than participating citizens, in the political system that has profoundly affected their standard of living and way of life since 1949.

At the outset Burns alerts the reader that the Communist party's domination of a centralized political system essentially restricted participation in rural China to attempts at affecting the implementation of policies decided elsewhere. This limited scope for participation shrinks further still as one follows Burns's discussion. He considers each of the officially sanctioned methods of political participation—village assemblies, mass organizations, and elections, as well as a variety of other means (oral and written) for directly expressing opinions or making complaints. Burns's work suggests that none of these alternatives have proven very reliable except in the unimpressive cases when peasant preferences coincided with those of powerful high officials or when those they tried to influence were largely indifferent. Thus, for example, Burns notes that "peasants were more effective" during elections in which "national leaders supported village elections" because they could be sure the result would be "the selection of local leaders who were sympathetic to policies favored in Beijing" (pp. 118–19). One must ask, In such a case is it the peasants or the national leaders who are effective? Similarly when Burns refers to villagers using visits by outsiders "to ensure that local leaders complied with central directives" (p. 138), one is led to ask, Who is using whom?

More impressive examples of political participation are discussed. They result, however, from peasants' resorting to means the party viewed as illegitimate (e.g., exploiting family or factional connections, withholding labor, bribing officials, or engaging in collective violence). The ultimate irony seems to be that genuine peasant participation in the People's Republic of China (PRC) has depended more on the residue of traditional methods of political influence than any of the revolutionary methods created and promoted by the CCP after 1949. This reinforces what has become a dominant perception of rural China in recent years. Early research that dealt with rural life in the PRC (e.g., by Franz Schurmann and A. Doak Barnett) had emphasized the massive institutional changes embodied in the regime's revolutionary policies. But beginning in the late 1970s, researchers (e.g., William Parish and Martin King Whyte, Elizabeth Perry, Anita Chan, Richard Madsen, and Jonathan Unger) have suggested instead the extent to which traditional patterns were preserved or even reinforced despite ostensibly great formal organizational changes. Consistent with this new consensus, the patterns of political participation Burns describes seem to reflect an uneven mix of traditional practices and imposed official constraints.

This book is designed to reach a broad audience. It begins with a general introduction to the subject of political participation (chap. 2) and a discussion of the institutional setting in rural China (chap. 3). The summaries of key concepts and facts in these chapters are accurate and thoroughly documented in what is a valuable bibliography both on political participation and politics in rural China. Nevertheless, specialists may find these unavoidably brief treatments of well-worn topics somewhat superficial.

A more troubling aspect of this book for political scientists, however, especially those not familiar with the literature on China, will be the author's use of evidence. In his appendix Burns candidly discusses his data base. The strength of his approach to the difficult problem of studying politics in a closed society is that he draws on diverse and independent sources of information. This is a partial safeguard against systematic bias introduced by relying exclusively on refugee interviews or official sources. But Burns might have made better use of the data he amassed. Absent a representative sample amenable to statistical analysis, he might have set forth several clearly stated propositions about the nature of participation in rural China at the outset (instead, some are suggested in the conclusion) and then examined his data for hard tests of these propositions. As it stands, the reader may wonder whether the data offered provide evidence relevant to the analysis or anecdotes whose representativeness depends on the judgment of the author. While his extensive training and research experience lead one to trust Burns's intuition, some may remain skeptical. Among the nonsinologists, skepticism will be heightened, perhaps unfairly, by the brief references to selections from short stories and statistics based on the statements of a small number of refugees.

AVERY GOLDSTEIN

University of Pennsylvania

Parties and Elections in Greece: The Search for Legitimacy. By Richard Clogg. Durham: Duke University Press, 1987. 268p. $38.95 cloth, $26.95 paper.

Political Change in Greece: Before and after the Colonels. Edited by Kevin Featherstone and Dimitrios K. Katsoudas. New York: St. Martin's Press, 1987. 301p. $37.50.

The Rise of the Greek Socialist Party. By Michalis Spourdalakis. New York: Routledge, 1988. 331p. $59.50.

These are three of the recently published books providing valuable information on continuity and change in Greek politics, particularly since the fall of the military regime (1967–74). Each discusses the major political parties, although Spourdalakis' focus in *The Rise of the Greek Socialist Party* is on the Panhellenic Socialist Movement (PASOK). PASOK was created by the charismatic current prime minister Andreas Papandreou after the fall of the junta and in 1981 became the first Socialist party to come to power in Greece. Spourdalakis was initially involved in PASOK but became disenchanted as evidenced by statements such as "The story of PASOK . . . constitutes a long chain of broken promises, undemocratic practices and dashed hopes" (p. 265). Focusing on the internal organizational structure, though considering external features, Spourdalakis explains why PASOK failed to reach its radical potential.

Featherstone and Katsoudas maintain in their edited book, *Political Change in Greece*, that there have been significant changes shaped by the parliamentary rule prior to 1967, as well as the military rule. This book is very broad, perhaps too broad, with contributors considering the setting, the party system, the wider political process, and external relations.

Although not reflected in the title, Clogg's *Parties and Elections in Greece* deals with parties and elections mainly after World War II, accenting events since 1974. While well-written, the jam-packed first chapter summarizing about a hundred years may be difficult for students expecting an introduction to the Greek political system. Clogg indicates he is neither dealing comprehensively with the political system nor attempting "to elucidate Greece's political culture and in particular the all-important issue of clientelism" (p. vii). While this would have been a major task, it seems essential to truly understanding the Greek political system.

Featherstone suggests that Greek political culture can still be contrasted with the rest of Europe. For Spourdalakis there is "an overwhelming petty-bourgeois flavor to the Greek art of conducting politics" (p. 7). This includes short-sightedness, acquisitive, conformist attitudes; and individualism—which make the formation of a mass democratic party difficult. Greeks relate to politics in an obsessive and highly unsophisticated manner, with politics rarely connected to social issues or social classes.

In Featherstone and Katsoudas' collection Dimitras notes that the prevailing attitude during the 1960s was anticommunist; however, following the collapse of the junta a profound generational cleavage emerged, with the youth being antitraditional, anticapitalist, and anti-Western. This represents the newest form of the historical conflict between modernizers and traditionalists in modern Greece. Attitudes toward the West have a cultural (secular) and political dimension; according to survey data there is little overlap between the two dimensions. Verney argues that anti-Westernism has always been present in Greece due to the influence of the Orthodox Church and the Byzantine and Ottoman Empires, while Clogg in his book suggests that "resentment at the way Greece has been manipulated, by external forces and powers outside its control is a deep-seated strain in the Greek psyche" (p. 143).

Each book recognizes that Papandreou and

PASOK capitalized on the growing anti-Westernism by stressing strong nationalistic appeals, such as "Greece belongs to the Greeks." In the edited volume, Coufoudakis presents the most in-depth analysis, noting a foreign policy consensus emerging after 1974 that cut across ideological and party lines and tended to be more independent and multidimensional. While Papandreou and PASOK articulated the criticism of U.S. influence unlike any other non-Communist party had, both the conservative New Democracy party (in power from 1974 to 1981) and PASOK discovered problems implementing an independent foreign policy. The Greek economic situation, Greek-Turkish relations, and superpower relations and attitudes all restricted the options available to the government (p. 237). PASOK adopted pragmatic operational policies that were pro-Western on core issues but offered symbolic declaratory assertions of independence on peripheral issues.

All three books also acknowledge that political clientelism persists, although its form may be changing. Mouzelis in the edited volume presents the most analytical discussion of this significant phenomenon. Until the junta, the major political parties consisted of extensive clientelistic networks controlled by politicians who were not completely dependent on the party structure or the party leader. PASOK broke the dominance of the clientelism-oriented politicians, bringing a change from a "decentralized personalism of the typical clientelistic party to the highly centralised personalism of the typical Populist party" (p. 278). With its extensive grass roots organization, PASOK can also systematically place its own people at *all* levels of bureaucracy instead of just the key positions.

A major controversy considered in each of the books is how to classify PASOK as a party. Spourdalakis contends that in order to understand what appears at times to be PASOK's erratic and often apparently contradictory policies, one must remember the trilateral political tendencies within PASOK (radical socialist, technocratic, and traditional clientelistic). Various authors agree that PASOK has moderated its positions over time, but the extent of the moderation is less clear. Spourdalakis sees the change as dramatic and provides documentation to support his view. For example, economic radicalism was abandoned, as illustrated by the change in rhetoric from "eliminating the exploitation of man by man" to "reducing income inequality" (pp. 164–65). PASOK's interest turned to convincing voters that it was a pragmatic and efficient party that could handle the nation's problems (p. 169). Yet to Featherstone (p. 125) PASOK is a party "committed to radical reform"; and to Calligas (in the same volume, p. 138) PASOK's principles were not abandoned, though extremism was toned down. Clogg is more equivocal, concluding that "Papandreou was able to sustain the impression that PASOK represented a radical new element in the political spectrum" (p. 213).

Each book makes it clear that it is Papandreou's charismatic personality that dominates PASOK. According to Spourdalakis, from early on Papandreou would not tolerate being accountable to the movement, so those favoring internal democratic procedures often found themselves outside the movement. Speculation is raised about the future of PASOK after Papandreou, suggesting possible fragmentation (Featherstone, p. 132; Clogg, pp. 142–43) or suicide (Spourdalakis, p. 284). Ultimately only time will tell on this issue.

In conclusion, while not a great deal is new in the edited volume or Clogg's book, they do provide good introductions to Greek politics. In several ways they are similar, but Clogg's is more organized, thorough, and focused, offering a wealth of material on the political system. Both are more descriptive than analytical and seem less negative about PASOK's desire to be electorally successful or its accomplishments within the Greek setting; while Spourdalakis presents an in-depth but critical case study of PASOK.

BETTY A. DOBRATZ

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Class, Ethnicity, and Democracy in Nigeria: The Failure of the First Republic. By Larry Diamond. Syracuse: Syracuse University Press, 1988. 376p. $39.95.

Nigeria is in the process of returning again to civilian and democratic rule, to a proposed Third Republic. Two earlier massive and violent failures of democratic rule raise the question, What went wrong? Diamond argues that the "Second Republic failed because the underlying causes of the First Republic's failure were never fully understood and clearly discerned" (p. 3). The book, a shortened version of his dissertation, presents Diamond's answer in hopes the design of the Third Republic will take account of the lessons argued.

Three introductory chapters state the themes and concepts for his argument, and present an overview of socioeconomic conditions and political events preceeding independence in 1960. The final chapter summarizes the argument and the implications of this study for theories of class and ethnic relations and the possibilities for democracy. Five intervening chapters focus on specific crises in the First Republic in 1962-65. Each historical chapter summarizes (succinctly, masterfully, and often with eloquence) the development of the crisis and then presents an analysis of the cleavage groups involved, the outcome and causes of the crisis, and its direct and indirect effects on the legitimacy of democratic rule.

The contribution of the study must rest on its theory, as the breakdown of the First Republic has been analyzed and argued extensively. Diamond extracts his descriptions and data from secondary sources published largely during the 1960s and 1970s. His basic argument is that democracy and the First Republic failed because of the absence of salient and continuous cross-cutting cleavages in Nigerian society. Existing ethnic divisions and suspicions; a lack of commitment by Nigerian elites to democratic norms; an inherited, unbalanced, "bizarre" (p. 135) federal political structure; and the cumulative effects of corrupt, violent, and demagogic partisan politics contributed to the breakdown of civilian rule—but the ultimate causes can be found in the general poverty of the country, the growing centrality and dominance of the state, and the development of a hegemonic, though regionalized, political class. The desire of the political class to gain control over the state in order to help itself materially in turn generated ethnic appeals to mobilize support, overrode commitments to democratic norms, encouraged corruption and arrogance, led to the entrenchment of sections of the ruling class in each region, and polarized all social cleavages along a northern (Hausa, Muslim, authoritarian) versus eastern (Igbo, Christian and pagan, competitive) chasm, ultimately leading to the ill-fated and tragic attempt at secession by Biafra. The western segments of the ruling class either became appendages of the northern feudal overlords (the Akintola faction) or were repressed legally and by pure force (the Awolowo group). The "faint rotation of cleavages" (p. 303) during the 1950s had clarified into antagonistic power blocs by 1965.

In the end, all potential cleavages (class, ethnic, religion, region, functional interests) had been manipulated to coincide, and the competition for resources (through control of the state) had turned from a political game played by rules into a "desperately instrumental" (p. 45) struggle for survival of regional class against regional class. The process of class formation, argues Diamond, required control of the state, and control of the state through democratic, electoral means required the evocation of ethnic sentiments as the most effective means of mobilizing mass support. Ethnicity, once mobilized, proved a two-edged sword and trapped both the ruling class and the masses in their regions—neither could transcend ethnic sentiments to arrive at a unified consciousness of its class position.

Diamond's argument brings together various theoretical strands of an emerging consensus among observers of the Nigerian scene—the shift in explanations of cooperation and conflict, crisis and stability from tribe to class (or interest group); the hegemony of the state in Nigerian society; the dominance of politics over economics (classes form by gaining political power and then use power to gain control over the means of production); the continuing hold of cultural traditions; and the absence of honest leadership. Diamond arranges his argument in a schema showing the "flow of causation in the failure of the First Republic" (p. 320). The schema is a succinct and insightful synthesis of a vast literature; it is suggestive, complicated (there are fifteen factors, each subdivided), and exhaustive.

Diamond clearly seeks to downplay the role of ethnicity (or tribalism) in explanations of Nigeria, and in that he is correct. Still, there are some inconsistencies in his linking of class to ethnicity. It is interesting how little class shows up in the consciousness and utterances of Nigerians and how much in the explanations of observers. Diamond puzzles, "If class, rather than region or ethnicity, was the most destructive cleavage, how could it not have been more apparent?" (p. 298). How could the ruling class have failed to transcend its ethnic bases? His answer is that conditions were not yet ripe for transcendence in the First Republic, though they have been changing of late and ethnicity has decreased in salience.

His argument that class is more important than ethnicity is unpersuasive on the evidence offered. First, his own argument that ethnicity is fundamental to class formation seems to assign more theoretical importance to ethnic than class identities. Second, class is defined on the basis of political power rather than economic condition (since power is the means to material wealth). Ethnicity is the way to power, hence to class. Third, class is defined by the motivations of individuals. If individuals seek "wealth, prestige and power for themselves" and have "individual &classé aspirations" (p. 60), they are engaged in class action; if they seek communal goals they are ethnically conscious. This distinction does not hold up, for ethnicity can be (as Diamond also argues) a personal, wealth, and power seeking motivation as well. There is an additional problem with using motivations as indicators of class or ethnicity. Nigerian leaders used ethnic terms to explain what they were doing, yet Diamond discounts those explanations and thinks they were really disguises for class motivations. Why should we accept his interpretation of what Awolowo, or Azikiwe, or Balewa really wanted?

Fourth, the conception of ethnicity used is not clearly enunciated. Diamond vacillates in his view of ethnicity. Sometimes he seems to accept the argument of the "primoridalism" modernization school that ethnic identities overpower rationality; at other times ethnicity is seen as a chosen identity, a pragmatic response to survival needs. It is also a view of ethnicity from the top, which fails to take into account the importance of ethnicity as a means of struggle by the masses. Ethnicity can be the most effective way for the powerless to gain a share of the national cake, small as the crumbs may be. Lastly, ethnic identities are fluid and situational but not wholly so. There are limits to the extent they can be manipulated; the failure of a "minorities" identity in Nigeria is an example. Minorities, if they ever coalesced, could be a major player in the political arena but that goal, despite efforts by cultural entrepreneurs, has never been achieved. Minorities have remained divided and peripheralized.

There is a last, and unavoidable, argument for the importance of ethnicity. All explanations of Nigerian life and society are always evaluated for their biases, and Nigerians will have no trouble perceiving Diamond's argument as a "southern" attack on the north. He has some very unflattering things to say about the cultural arrogance and pride and feudal manners of the northern aristocracy and places much of the blame for the collapse of the First Republic at the palace doors of northern rulers. He is correct in his analysis, yet his class analysis will reinforce ethnic beliefs if read in Nigeria.

In short, the articulations of class to ethnicity are not established persuasively. The theoretical failure consists in the attempt to see one identity as more important than the other and not both as equal, theoretically independent values, interests, motivations and objective conditions. Despite its unresolved theoretical ambiguities and because of its scope and ambition—to explain both Nigeria and democracy—this book will stand as a richly informed, theoretically sophisticated, and elegantly written and argued introduction to, and summary of, the fate of democracy during the formative years of the Nigerian republic.

OTWIN MARENIN

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Labour and Development in Rural Cuba. By Dharam Ghai, Cristôbal Kay, and Peter Peek. New York: St. Martin's, 1988. 141p. $45.00.

This small volume is part of a continuing research program on employment and poverty problems undertaken by the World Employment Programme of the International Labor Organization (ILO). It discusses the changes that have taken place in the rural sector in Cuba between 1959 and 1985. It is based on data collected during visits to state farms and production cooperatives, in discussions with planners, ministry officials, the Small Farmers Association, and so on and from published and unpublished materials on the Cuban economy.

The first chapter provides an overview of changes in land distribution patterns, agricultural organization, wages, incentives, and so forth from 1959 to 1970. Among those that would have interesting ramifications in later decades was the expansion of the peasant sector through two agrarian reforms (around 150 thousand people received land titles). By the mid-1960s this group held 43% of Cuba's arable land (p. 14).

The middle section of the book discusses the economic reforms instituted after 1970 and Cuba's three principal forms of agricultural production: state farms, cooperatives, and individual private farms. In the last two decades the state farms, which account for an overwhelming proportion of the agricultural workforce employed, agricultural output, and land incorporated, have undergone many changes. Two of the most interesting are *autoconsumo* (whereby a portion of the farm's output is produced for consumption by farmworkers' families and in their workplace cafeterias) and decentralization. One of the main vehicles for decentralization has been the creation of 50–200-member Permanent Production Brigades, which have attempted to tie wages more closely to output and to increase workers' participation in decision making. The formation of these production units has been considered important enough that the Communist party, the Young Communist League, and the unions have all been reorganized to conform to the Brigade system.

Cuba's individual private farmers, many of whom were created and have been maintained by the postrevolutionary government, are a dwindling and aging group. Nevertheless, the authors are correct to point out that what they lack in quantity they make up for in high quality production. Their relationship to the Cuban government has generally been good; they have not been the targets of coercion and their production quotas are not high, leaving them able to produce considerable amounts for their own use and, for a brief period, for sale to consumers.

Since the mid-1970s the government has tried to attract private farmers away from individual plots by encouraging the formation of cooperatives. This "middle ground" (p. 69) form of agricultural organization has grown rapidly. From 44 cooperatives, with a land area of 6,052 hectares and a membership of 365 in 1977, the cooperative sector in 1984 encompassed 1,414 co-ops, over one million hectares of land, and over 72 thousand members. The inducements to join are many: improved social, medical, and educational facilities; privileged access to credit and machinery; attractive prices for land contributed by members (by 1981 64% of the co-ops' land was acquired this way [p. 71]), and a great deal of power over decision making. Co-op management is elected, major decisions must be approved by the members assembly, and norms are set through a flexible committee system. The co-ops retain most of their profits, which are used to pay wages and dividends, to reimburse members for land and capital contributions, and to finance new investments and recreational and social services.

The two concluding chapters assess the results of Cuba's rural sector policies over the preceding 26 years and highlight what has been distinctive about them. Unfortunately, the chapters are somewhat repetitive and the rural sector focus could have been sharper. Nevertheless, the authors provide a succinct summary of the revolution's impressive social and economic achievements. Among these are increasing annual growth rates since 1959, declining export orientation measured by export-to-GDP ratios, steady increases in agricultural production, spectacular increases in mechanization of sugar cultivation, a Gini coefficient of .26 (p. 123), together with educational, health, and social security advances of which Cuba is understandably proud.

The authors are clear about what has been responsible for these successes, including social ownership, food rationing, dramatic reductions in unemployment and increases in social consumption, prioritization of agriculture, the development of human resources, and the cultivation of a close relationship with the Soviet Union. However, they leave unexamined the question that would have allowed them to make a substantial contribution to the understanding of development, Latin America, socialism, and comparative politics: How was Cuba able to institute these policies *politically?* What has been distinctive about the configuration and interaction of social interests on the internal (and external) political stage that enabled Cuba to pursue this development course with such notable success? Why have the political conditions underlying these economic and social achievements in Cuba been so hard to replicate elsewhere?

Labour and Development in Rural Cuba accomplishes its purpose of informing the general reader about the rural sector in postrevolutionary Cuba, a topic that has not received adequate scholarly attention, especially since it has been so central to Cuba's development efforts. It provides useful reference data in an accessible format. However, especially those readers with some background in development, socialism, or Latin America will wish it were less descriptive and more analytically and theoretically informed and that it treated more topics in greater depth.

LINDA FULLER

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Democracy in Latin America: Colombia and Venezuela. Edited by Donald L. Herman. New York: Praeger, 1988. 344p. $45.95 cloth.

Given their common cultural and political heritage, the countries of Latin America are, or should be, a natural arena for the comparison of most-similar political systems. Yet on the whole, Latin Americanists (notably political scientists) have taken relatively little advantage of such affinities. Although there are some notable exceptions, such as Daniel Levine's *Religion and Politics in Latin America* (1981), the vast majority of works on the region consist of case studies of single countries with only passing reference to other hemispheric countries—or of overviews seeking to generalize about Latin America as a whole.

Therefore a book that seeks both to address a currently salient and important topic such as democracy in Latin America by looking concomitantly at two of the principal democracies of the region is very much to be welcomed. Indeed, Venezuela and Colombia have in common, in addition to the fact that they are both Latin American (and Andean), the circumstance that they constitute two of the three countries (the other being Costa Rica) where procedurally democratic institutions have now survived for at least three decades, their origins preceding the recent spate of democratization (or redemocratization) in the rest of the region.

The editor, Donald L. Herman, has assembled a group of authors who are among the most knowledgeable about Colombia or Venezuela or both. Topics emphasized include political parties (Gary Hoskin, John Martz), insurgency (Daniel Premo), the economy (R. Albert Berry and Francisco Thoumi, Robert Alexander), foreign policy (William Hazleton) and U.S. influence (David Myers). There are also essays that offer general assessments of changes, trends, or crises in one or the other country (Harvey Kline, Diego Abente), a good chapter on the Colombian church by Alexander Wilde, and a data-based chapter on public opinion in Venezuela concerning democracy and the role of the military (by Enrique Baloyra). The editor provides an introduction and conclusion, in which he stresses the fusion of monistic and democratic traditions in polities like the Venezuelan and Colombian; however, there is no real attempt to pull the disparate chapters together.

If there is a central (or recurring) theme to the book, it might be the relative optimism of most of the authors concerning the state of democracy in the two countries in spite of such problems as drug traffic and insurgency, especially in Colombia, and signs of some popular apathy or disquiet with regard to the functioning of democracy in both countries. In an argument with considerable potential relevance for the rest of Latin America, several authors stress the role of leadership and of consociational arrangements in establishing democracy in the two cases at hand. The inference is that political engineering can in substantial measure substitute for the dearth of more deterministic economic and sociological conditions of democracy in Third World nations.

Nonetheless, while *Democracy in Latin America* is a generally useful, up-to-date compendium by a group of able scholars, it has two notable limitations. First, in a volume of over three hundred pages, several topics seemingly of considerable importance for a discussion of democracy have been slighted, with no separate chapters devoted to them and only occasional allusions or brief mentions in the other chapters. Such topics include the military (apart from Baloyra's report on Venezuelan public attitudes toward it), the role and behavior of interest groups, and the policy-making process (other than foreign policy).

The second significant failing of the book is that even while two countries are brought together under the rubric of one book and all the essays are in some sense relevant to the recent performance of democracy there, the discussion of them in the main constitutes juxtaposition more than it does true comparison. This even tends to be true of the several chapters that explicitly include both countries. For example, we get little attention to the differences between the party systems of Colombia and Venezuela or to the reasons for those differences, and none to the Venezuelan church and the consequences of its weakness compared to the church in Colombia.

We have, then, in *Democracy in Latin America,* a book that encompasses largely descriptive accounts of important aspects of the politics (and to much lesser extent, the economies) of two of Latin America's democracies yet a book that takes only very modest steps toward the systematic comparison for which the Latin American area, in particular, should afford fertile ground.

ROBERT H. DIX

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The Party and Agricultural Crisis Management in the USSR. By Cynthia S. Kaplan. Ithaca: Cornell University Press, 1987. 183p. $27.95.

This book is a useful addition to the sparse literature on policy implementation in the USSR. Cynthia Kaplan focuses on the primary administrative unit, the *raion* (district). She demonstrates that the activities of district party committees (*raikoms*) in primarily industrial Leningrad *oblast* (province) profoundly differ from the functioning of *raikoms* in primarily agricultural Rostov *oblast.* By a painstaking collation of the scattered data in published Soviet sources and dissertations, she generalizes from these cases to argue that *raikoms* in industrial *oblasts* behave basically as staff institutions, offering indirect support to powerful industrial managers responsible to the province party committees or their own centralized industrial ministry superiors. *Raikoms* in agricultural provinces, on the other hand, are line managers, highly interventionist and mobilizational "crisis management" agencies that directly involve district party officials in micromanagement of the farms.

Kaplan's introduction implies that this finding contradicts Jerry F. Hough's argument about the usual interventionist role of Soviet provincial party committee (*obkom*) secretaries in his classic, *The Soviet Prefects* (1969). However, since she studies a lower-level party agency than Hough, her findings can equally well be made consistent with his.

The body of the book examines two questions: Why is there such a difference in the functioning of party agencies in agricultural and industrial areas? How does the crisis management mode of party activity in the countryside operate in practice? A concluding chapter argues that the difference in management style is eroding as a consequence of social change in the countryside. The final chapter speculates on what this shift in party methods indicates about the Communist Party's (CPSU) future role in running an increasingly complex, differentiated society.

The crisis management style grew out of the Soviet regime's post–World War II need to maximize immediate food supply deliveries to the cities and the rural party's weakness, Kaplan says. Neither the sparse *kolkhoz* party membership nor the raw rural specialists, constantly pulled toward better jobs in the city, were competent to manage the collective farms. As a result, *raikoms* came to manage production by ratifying managers' decisions, frequently firing them for failure to fulfill production plans, and superseding farm managers altogether through "plenipotentiaries" sent out from the *raikoms* to run the farms directly during peak work periods.

An additional explanation for the development of "command farming" that gets little attention in the book is the absence of economic incentives for collective agricultural production. In 1947-51, the period of Kaplan's most detailed research, state delivery prices repaid only a tenth of collective farm *(kolkhoz)* expenses on grain production, a twentieth of meat production expenses, and a fifth of milk outlays (*Kommunist* no. 1 (1988); 103). As a consequence, both agricultural workers and specialists were ill paid, living from the produce of their private plots. Relatively well-off industrial workers and particularly industrial specialists would have been more responsive to economic incentives, in particular bonuses for plan fulfillment. While industrial district party committees, then, would have been able to make greater use of "indirect" economic mechanisms to control production, the lack of economic incentives for agricultural workers had to be compensated for by coercive, mobilizational ones.

Although the book is basically about the postwar period, Kaplan suggests that the late-Stalin-era rural order is basically like that established by collectivization in the 1930s. Following Kendall Bailes's argument about the technical intelligentsia, she says the cohort of expert industrial managers whose emergence limited the industrial party *raikoms* to a staff function developed in the late 1930s and early 1940s. Rural party committee crisis management, a relic of the party style of work from the first five-year plan, when it was normal throughout the economy, survived because of World War II's devastation of the countryside.

Kaplan assumes that crisis management remained the dominant style of *raikoms'* work in rural areas until the early 1970s, when Leonid Brezhnev's agricultural reforms began to effect the "industrialization" of the countryside. Then, as a new generation of better-trained party cadre and more mechanically skilled agricultural workers emerged, the party *raikoms* could shift away from coercion and direct intervention in production. As they moved from line management to staff functions, the rural district committees' operations would, presumably, come more and more to resemble party committees' activities in industrial areas. Kaplan anticipates this change and marshals sociological evidence to demonstrate that it is occurring, but her book does not discuss its implications in great depth.

This lack may be a matter of timing. Since the early 1980s, a great deal of pressure has been exerted on local party organizations in the countryside to change their methods of work away from crisis management. Organizational changes, including the now-dying district agricultural associations and district Agro-Industrial Combines and "agrofirms" have been repeatedly introduced. Although intended to force party committees to aid farmers rather than simply ordering them around, there is not much evidence that they have succeeded.

In the last three years, the CPSU has begun to change its agricultural policy in a quite-unexpected direction. Instead of trying to "industrialize" the countryside so that agriculture becomes factory work that happens to be performed outdoors, the regime is now a good way toward admitting that agricultural work is irreducibly different from industrial labor. This still-tentative policy seeks less to run rural areas like industrial regions than to do away with any party-state role commanding agriculture, in favor of marketized relationships. *If* this movement toward the market succeeds, the party as a whole will be looking for a new role in Soviet society, and differences between agricultural and industrial party organs will no longer matter very much. Such changes were not predicted by any Sovietologist when Kaplan was doing her work, however, and their success is still very much in doubt.

The book is a valuable examination of agricultural administration in the USSR, the first such study in almost 20 years. Although its conclusions may be overtaken by events, it is a worthy companion to Robert F. Miller's *One Hundred Thousand Tractors* (1970) and other standard works.

DON VAN ATTA

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The KGB: Police and Politics in the Soviet Union. By Amy W. Knight. Winchester, MA: 1988. Unwin Hyman. 348p. $34.95.

With the publication of Amy Knight's book, the academic study of the Soviet organs of repression and state security enters a new phase. Her book focuses on the state security system as an enduring institution, indispensable for the preservation of the rule of the Communist party from December 1917 to the present. As the subtitle indicates, Knight's main concern is the relationship between state security organs and the party leadership from the days of "Iron Felix" Dzerzhinskii to the present era of *glasnost*, General Chebrikov, and his new successor.

Knight first reviews the origins and history of the state security apparatus, noting briefly its czarist predecessors. Part 2 of *The KGB* explains the structure of the organization under its various titles and the nature of Communist party control over it. The career patterns of KGB officers are also traced. A final part of the book covers the various functions the KGB performs: internal security and antisubversion operations, protection for Party leaders, guarding the borders of the USSR, control over the political reliability and security of the armed forces, and a role in the implementation of foreign policy.

Knight's emphasis is always on the KGB as an institution, its personnel, the nature of its functions and its relations with other Soviet organizations, particularly the Communist party. She almost entirely avoids examples of Soviet operations. Instead, she describes the various elements and functions of the KGB without getting down to cases.

By doing so, she may be trying to avoid the charge of just telling more "spy stories" and consequently to win academic acceptance. The lack of attention paid in scholarly works to state security has meant that, as Knight amply demonstrates, many analyses of the Soviet regime have failed to consider the role of a vital organ in the Soviet body politic when assessing the nature of the beast. Nobody, since Solzhenitsyn, can overlook de*kulak*ization and the *Yezhovshchina,* but until Andropov came to power few commentators paid serious attention to the KGB as a factor in Kremlin politics or in the preservation of Soviet power. We welcome Knight's book for many reasons, one being since its publication, the role of state security in the Soviet regime throughout its history can no longer be ignored.

Knight sees the principal roles of the KGB as first, the sword and shield of the revolution (its traditional role); second, a counter to, and preventer of, dissent; and finally, a polling and social research service providing the Communist party leaders with a substitute for the free press and free speech that it is the business of the KGB to suffocate.

The KGB concludes with a series of useful appendices. They cover organizational nomenclature, extracts from the criminal code of the Russian Soviet Federal Socialist Republic (a particularly important inclusion) and listings of leading officials in the history of state security. In addition, Knight provides the interested reader with a series of suggestions for further reading.

By focusing on institutions, the bureaucracy, and the analysis of functions, Knight does slight some of the events in Soviet history where the KGB or its predecessors did play a crucial role. The changes in the bodyguard system reported by Peter Deriabin in his *Watchdogs of Terror* (1972) and analyzed in A. Avtorkhanov's *Riddle of Stalin's Death* (1970) in the two years preceding Stalin's death are major events in Soviet history, and the state security service was in the middle of them. Knight says only, "During the early 1950's, [the body guard detachments] were a point of focus in Kremlin power struggles" (p. 224). She also passes over lightly the role of Alexander Shelepin (the former chairman of the KGB) and Vladimir Semichastnyi (the then KGB chief) in the ouster of Khrushchev and Shelepin's campaign to achieve highest power.

The political role of Yuri Andropov, on the other hand, is well covered. Perhaps he learned from Shelepin's example that one must go slowly or risk provoking the other peers of the realm to opposition. In any event, he rose to power based on his command of the KGB. And Gorbachev rode to power on Andropov's back. Today's news reports the replacement of KGB chairman Chebrikov by General Kriuchkov, the KGB deputy who has headed the foreign operations directorate for the last 12 years. As Knight points out, Kriuchkov was Andropov's boy since 1954. Her exposition of the careers of senior KGB officers and the nature of the role the organization is now playing make it clear that by and large, the KGB is on Gorbachev's side. The most recent shifts in power in the Kremlin make *The KGB* more, not less, relevant to the unfolding saga of *perestroika.*

DONALD F. B. JAMESON

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The Peoples of the Soviet Union. By Viktor Kozlov. Bloomington: Indiana University Press, 1988. 262p. $37.50.

One point should be made clear at the outset: this book, written by a professor at the Institute of Ethnography, USSR Academy of Sciences, does not reflect the political "new thinking" promoted by the Gorbachev reforms. First published in the Soviet Union in 1975, and then issued in a revised edition in 1982, *The Peoples of the Soviet Union* represents a fairly typical academic product of the Brezhnev period, now called the "period of stagnation." In this ethnodemographic and ethnogeographic survey of the population of the USSR, one encounters an unreal world in which there was no artificial famine in the Ukraine and no Gulag Archipelago and in which the problem of Jewish emigration is passed over in silence, while religion is believed to have ceased being a factor among the peoples of the Soviet Union.

What makes all of this unsettling is that the author apparently had an opportunity to revise his text for the English edition. Instead of so doing, he appears to have limited himself to updating a few of his tables and to making brief reference, on the last page of the book, to the important changes presently taking place in the USSR. In his balanced introduction, Michael Rywkin of New York's City College notes that Kozlov's institute "has failed to take full opportunity of Mikhail Gorbachev's policy of *glasnost*. . . . For the present, the Institute of Ethnography seems to have opted for caution" (p. 9).

In light of the timidity underlying Kozlov's text, one wonders whether it was in fact necessary to translate this book into English. A new all-union census is scheduled to be conducted in 1989 (the last one having occurred in 1979), and it might have made sense to wait for the new data to come in, and to give Kozlov a chance to reorient and restructure himself.

This is not to say that the book lacks value. As Rywkin points out, the study's ethnodemographic projections for the year 2000 are of considerable interest. Kozlov believes that by the year 2000 Eastern Slavs—Russians, Ukrainians, and Belorussians—will slip from constituting three-quarters of the Soviet population, as they did in 1959, to making up only two-thirds of the total. Ethnic Russians, who comprised 55% of the population in 1959, will number approximately half the population in the year 2000. By the next century, the growth rate of Eastern Slavs should approximate the very low rate of the Balts, and it is possible that the numerical growth of Latvians and Estonians will altogether cease by the year 2000. These growth patterns could result in severe labor shortages in the Russian Soviet Federal Socialist Republic and the European republics.

Kozlov is concerned about the growth rate among Soviet Muslims. While he predicts that that rate will slow down by the year 2000, it will nevertheless greatly exceed that of the European peoples of the USSR. This will result in the ethnic composition of the southern republics' becoming more homogeneous, and in the use of Russian's declining in these regions, including their urban areas.

It should be stressed that Kozlov is not a neutral observer and reporter of ethnodemographic processes. On the contrary, he is a fairly militant advocate of the formation of a denationalized, Russian-speaking "Soviet people" (*sovetskii narod*). Present patterns of urbanization, assimilation, and intermarriage are, he believes, leading to the gradual emergence of this new nonethnic entity. But this process should, he maintains, be accelerated by the economic policies of the Soviet government, and by a new approach to census taking. "A Karelian of Chuvash," he writes, "who has lived among Russians for a long time, and has forgotten his former language and culture may realize that he has effectively lost his former ethnic identity but at the same time has not become a 'real Russian.' . . . It is possible that such individuals would consider themselves part of the 'Soviet people,' but there is also no provision in census instructions for such consciousness" (p. 219). In a recent article appearing in *Moscow News*, Kozlov argued that the category of nationality should be omitted from Soviet internal passports. This would presumably facilitate the emergence of a denationalized "Soviet people."

Recent developments in the USSR appear to be rendering Kozlov's proposed program utopian. National self-awareness is booming among the peoples of the Baltic region and in the Transcaucasus, and there are indications that the phenomenon is spreading to the Eastern Slavs as well. There is at least a possibility that this upsurge of nationalism could affect the stagnant birth rate of Balts and Slavs, though this remains to be seen. In any case, Kozlov's views on ethnic processes in the USSR may already be outmoded.

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When Parties Fail: Emerging Alternative Organizations. Edited by Kay Lawson and Peter H. Merkl. Princeton: Princeton University Press, 1988. 596p. $75.00 cloth, $19.95 paper.

For many years scholars have been concerned about—or at least interested in—what has been happening to parties and party systems in Western democracies as well as in developing and authoritarian systems. Party systems do change, suddenly or gradually, minimally or fundamentally. One of the first to attempt to systematize party system change was Duverger in his 1951 book, in which he describes four basic types of party system "development": alternation, stable distribution, leftism, and domination. The special characteristics of political systems that contributed to differential party development were emphasized by Duverger. His type of system theorizing, despite certain weaknesses, should point the way for us.

When Parties Fail is useful even though it does not contribute as much to our knowledge of party *systems* change, as we might like. It is a book that brings together the work of many scholars in discussing 18 cases of party "failure" and the emergence of new political groups as a consequence of such failure. These cases range from the Communist party of Kerala, India, to the Social Democratic party in Britain, the National Democratic party of Alabama, the Greens in West Germany, the Sanrizuka in Japan, the Gush Emunim in Israel, and many others.

The aim of this collection of studies is to explore the political organizations that have been emerging to take the place of declining major parties. It is a valuable collection of information about alternative political movements and structures. New political groups in 14 different countries are examined. Several questions are posed in each case: Is there evidence of decline of parties? For what reasons? How do new political groups seek to respond to these party system failures and with what success? Above all, do such new organizations endure?

In addition to the descriptive contribution, the book does attempt to move toward some theoretical generalizations. It does classify these alternative organizations into four types: environmentalist, supplementary, communitarian, and antiauthoritarian (such as Solidarity in Poland, the Independence movement in Taiwan, and the coup groups in Ghana). In addition, there are two excellent theoretical essays that use two different theoretical approaches to explaining the causes for, and consequences of, the emergence of alternative organizations. Kay Lawson develops the argument that the deterioration of the linkage function of parties (i.e., linkage between citizen and state) is responsible for new parties and other new movements. She argues that new groups emerge where linkage is not well performed and feels the evidence from these case studies supports that hypothesis. On a second proposition—that new groups endure by providing such requisite linkage—she admits the evidence is not confirmatory. Peter Merkl writes a concluding piece relating the emergence and impact of these new groups to different types of party systems. It would have been of value if he had developed his insight that the different "forms of alternative action strikingly reflect the dilemmas of life and politics of these societies" (p. 586) more systematically.

The authors in this book do not all agree on what constitutes party failure. The majority seem to feel that "major parties fail when they do not perform the functions they are expected to perform in their own society" (p. 5), even though such parties may reform and survive. Two authors, Rose and Mackie, disagree, arguing that parties either persist or disappear and that the only proper definition of failure is disappearance. They argue 23% of the major parties in this century have disappeared (i.e., those which had been definitely institutionalized). The conceptualization of the phenomenon as "failure" is arguable. And perhaps the focus might better be on the concept of "party system change"—its patterns, processes, and systemic relevance—and on the conditions under which major parties adapt to change, and when this is successful and when it is not.

Nevertheless, the book is interesting, its central theme is challenging, its case materials valuable, and its bibliographic references often useful.

SAMUEL J. ELDERSVELD

University of Michigan

Book Reviews: Comparative Politics

China Builds the Bomb. By John Wilson Lewis and Xue Li-tai. Stanford: Stanford University Press, 1988. 329p. $29.50.

On 16 October 1964, a poor peasant China accomplished a high-tech "mission impossible" by detonating its first atomic bomb. Almost a quarter of a century later, Lewis and Xue accomplished an intellectual mission impossible by telling (almost) everything we wanted to know but were afraid to ask about the Chinese bomb. If there is a major fault line in this ground-breaking work, it is the overly ambitious objective of addressing multiple audiences and multiple issues. The authors have spent a decade to produce nothing less than an authoritative, complete, and compelling account of what really happened in politics, industrial technology, military thinking, high science, and national values in the long Chinese decade (1953-67). Inevitably, the book of this nature and scope entails a "too much, too little" problem for each of its multiple audiences. There is too much and too little for historians, political scientists, world politics analysts, hardware and software nuclear engineers, and, alas, even for "crazy" states in their nuclear status drive.

The study of Chinese foreign policy is generally known for empirical richness rather than for analytical rigor and theoretical innovation. The contribution of a book such as this can never transcend the kinds of bibliographical, conceptual, and theoretical resources utilized to describe, understand, and explain Chinese political behavior. When the authors began a decade ago this international collaborative quest to answer how and why China built the bomb, they must surely have had a "uranium challenge" of a tall order. As if to help overcome this challenge, the Chinese began in the mid-1980s to lift the veil of secrecy by releasing Marshall Nie Rongzhen's memoirs, an authoritative history of the nuclear industry, and a flurry of articles and essays. Compared to what we know about the U.S. and British nuclear programs, there are still large gaps in the Chinese data, the authors remind us; but the Chinese have been far more forthcoming in releasing nuclear information than either the Soviet Union or France. In any event, the authors have extracted, enriched, and synthesized from the newly released Chinese information (and U.S. diplomatic documents for 1949-57), field interviews, and even personal correspondence with some key figures in the Chinese nuclear program.

The superpowers loomed large in the conception, development, and final success of the Chinese bomb. U.S. nuclear threats were an initial catalyst, a necessary but not a sufficient condition for the success. Ultimately, the program saw the light of day through "national will." What energized this national will is the national consensus that in a nuclear world China without the bomb does not count or could not really stand up. This national will was well reflected in Foreign Minister Chen Yi's statement that China had to build the bomb at any cost, "even if the Chinese had to pawn their trousers" (p. 130).

Paradoxically, it can be said (and the authors do not say this) that Mao's dictum of the superiority of man over weapons did the trick. Soviet help was an important but not a decisive factor, "an abbreviated textbook for future learning and application" (p. 222). Beijing's nuclear quest dovetailed its changing relations with Moscow, evolving from dependency (1955-58) to interdependency (1959-60) and finally to self-reliance (1960-64). Mao's dialectics also reflected and effected the developmental process: Mao demanded and resented Soviet nuclear help. In spite of, or perhaps because of, Soviet withdrawal of nuclear assistance, the program managed to forge a coalition of the high priests of politics and science. In the end, it was not a national "tragedy" (as President Johnson pronounced to the world) but a national triumph. Nie Rongzhen's organizational genius helped too, as he was able to order a high level of redundancy in procurement, design, and development.

The book is long on developmental details (four of the nine chapters) and short on strategic doctrines (only one chapter). The black holes in China's strategic thinking are better delineated than explained. The authors skate over the changing strategic doctrines (and the global implications of China as a wild nuclear card) with a let-the-nuclear-facts-speak-for-themselves approach, as it were: "The weapons, once deployed, spoke for themselves" (p. 211). Even newly discovered facts can never speak for themselves nor by themselves offer an explanation. Chapter 8, "Strategic Doctrines and the Hydrogen Bomb"—akin to an epilogue keyed to the hydrogen bomb—is something of a letdown.

Still, this is scholarship of a high order. It neither chants nor condemns Beijing's official script but presents a thoroughly substantiated story of how and why China decided to go nuclear. As well, it is easily one of the best and most meticulously researched studies of China's superpower relations in print. On the larger and more elusive question of the black box of strategic decision making, this study lights up the road ahead for moving the field of Chinese foreign policy toward a theory that would relate disparate variables into a more comprehensive picture of Chinese international conduct. A Sino-American scholarly collaboration of this kind is easier promised than performed, but this work is a testimonial to its feasibility and desirability.

SAMUEL S. KIM

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Public Opinion and Political Change in Poland, 1980-1982. By David S. Mason. Cambridge: Cambridge University Press, 1985. 275p. $42.00.

It should not surprise anyone that the political crisis in Poland in 1980–81 has attracted considerable attention among Western scholars. While a comprehensive bibliography of books, chapters, articles and other pieces analyzing different aspects of the crisis—comparable to that dealing with the "Prague Spring" in Czechoslovakia in 1968—has not yet been published, I suspect that it would cover well over a thousand items. In light of it, one might rightfully expect that every important facet of the Polish 1980–81 crisis has already been thoroughly covered and that any new study would simply recycle old verities ad nauseam.

I am pleased to report that my suspicions proved totally unfounded in the case of David Mason's examination of the process of political change in Poland during 1980–81 with the help of a variety of public opinion polls. It is a welcome addition to the already-impressive volume of literature devoted to this particular topic. Although the study does not offer any fresh or illuminating insights into the origins or consequences of the crisis, its major virtue lies in its approach which differs rather strikingly from that used by other authors who investigated the same events. Whereas the latter tended to focus primarily on key components of the Polish political systems, such as leadership, groups, institutions, policies, and ideology, Mason decided to concentrate on the Polish public and its perceptions of the changes that have been taking place in Poland in the early 1980s. This, to repeat, is a highly innovative approach that has one additional merit in providing an empirical confirmation of many conclusions about the crisis that for a long time resembled conventional wisdoms based largely on personal impressions and intuitions of individual observers of the Polish scene.

In making a felicitous choice of approach, the author was greatly helped by the availability of public opinion data resulting from surveys conducted by Polish poll takers. As pointed out correctly by Mason, "Poland has the richest tradition in sociological research in Eastern Europe"; and if I remember correctly, the first attitudinal survey of Polish students was conducted in 1957, more than 30 years ago. Some of the surveys initiated in the early stages of the crisis in November 1980 have been continued until the present; and, hopefully, someone will someday take advantage of the available data base to produce an exciting sequel to Mason's study.

In addition to the approach, I also liked the structure of the study, which is partly chronological and partly topical. Mason mercifully spared his readers another blow-by-blow account of the crisis and instead succeeded admirably in blending a discussion of key events with an examination of popular perceptions of these events that strengthened his own interpretation of the depth and breadth of the sociopolitical changes.

This is all good, even very good, news. What about bad news? I really could not find any serious weaknesses in Mason's study, and whatever minor criticisms I may have are really in the nature of nitpicking. Thus, I would take issue with the author's conclusion that "Poles supported, or at least accepted, many of the institutional and ideological features of the postwar political system," including the principles of socialism (egalitarianism and social welfare) and the leading role of the communist party (p. 239). I would argue that while there may have been a general approval of basic principles of a welfare state, the successive crises of 1956, 1968, 1970, and 1976 have provided a good testimony to the popular belief in the illegitimacy of the communist system.

Mason also seems to blame Solidarity for giving preference to political issues at the expense of the economic ones (p. 240). I would suggest that apart from the fact that a separation of political from economic policies tends to be an artificial one, Solidarity leaders may have realized that the economic situation in the country in 1980–81 was beyond redemption and that by trying to do something about the economy, they would lose even more popular support than they actually did.

Although I applaud the author's frequent use of survey data, one caveat is in order. Many of the most recent surveys suggest that a majority of Polish respondents tended to be quite inconsistent in their answers, welcoming on the one hand the proposed far-reaching economic reforms but on the other hand opposing many of the features (rising prices and rents, unemployment) that such reforms were bound to generate.

Finally, how do the author's conclusions stack up against the current changes in Poland? In general, Mason is quite cautious in predicting the country's future. While he concludes that "the institution of Solidarity is moribund, . . . the idea of Solidarity is alive" (p. 248), he also believes that "in retrospect, though, 1981 may prove to be a catharsis for Poland, in that the revolutionary strain may have run its course" (p. 246). The fact that he has proved wrong in his predictions should not detract from the impressive contribution he has made to our knowledge of Polish polity and society.

ANDRZEJ KORBONSKI

University of California, Los Angeles

Split Signals: Television and Politics in the Soviet Union. By Ellen Mickiewicz. New York: Oxford University Press, 1988. 286p. $22.95.

Mikhail Gorbachev's policy of *glasnost* has revolutionized not only the published word in the Soviet Union but also the broadcast media. With more varied, candid, and interesting programs available, Soviet citizens are increasingly turning to television for their main source of news and information. In 1960 only 5% of all Soviet citizens had access to TV, but today television reaches more than 93% of the public. The Gorbachev leadership, more than any previous regime, recognizes the powerful impact TV can have in mobilizing the people in support of economic and social reforms.

Mickiewicz has provided us with a fascinating study of Soviet television. She begins with a thorough description of the growth and development of TV, radio, VCRs and other mass media in the USSR. Mickiewicz notes in particular that the primary function of Soviet television is to educate and socialize values in Soviet citizens, rather than primarily to entertain the public. This naturally affects what is deemed newsworthy in the Sovit Union. Until the introduction of *glasnost* it was considered improper to focus on negative events such as natural disasters or accidents. Even with major changes under Gorbachev, celebrity gossip, horoscopes, social columns and other kinds of fluff do not appear in the Soviet media.

Mickiewicz provides a comparative content analysis of more than a hundred hours of Soviet and U.S. television news. She confirms that Soviet television devotes substantially more attention to international affairs (esp. the United States) than do most Western media. On the other hand, a relatively large percentage of Soviet news coverage consists of reports on official visits and other largely ceremonial occasions. Soviet television also devotes much more time to reporting on economic matters.

With the advent of *glasnost* Soviet viewers are also being treated to more than one perspective on issues. A number of programs have aired using a roundtable format to discuss a wide array of political and social questions. The author quotes portions of these exchanges to illustrate the range of views being presented. Since the book was written, even more startling changes have occurred in the range of permitted discussion of political, economic, and social issues.

Mickiewicz also analyzes the impact of television on Soviet citizens. She notes that the youth and the elderly are becoming increasingly dependent on television to saturate their free time. As in the West, educators in the Soviet Union are concerned about the impact of television viewing on Soviet students. All the consequences of television are not negative, however. Mickiewicz notes that in a country as large and diverse as the Soviet Union, television can play an integrating role.

Especially evident is the use of television by General Secretary Gorbachev to bolster his image and support among Soviet citizens. The author notes that television is a more credible medium than others and is functioning not simply as a channel to communicate the leadership's policy preferences but also as a means for citizens to participate in ways that reinforce the leadership's goals. But, as in other areas, split signals are being sent. On the one hand, the media is being used to persuade people to work harder and more honestly and support the Gorbachev reforms. On the other hand, *glasnost* involves expanding the opportunity and ability of citizens to articulate demands and express their own preferences. The tensions and conflicts inherent in these mixed signals remain unresolved.

Split Signals is a detailed and thoroughly documented work. The author uses a wide variety of resources, including extensive references to Soviet materials on the media, interviews with Soviet officials, as well as a content analysis of Soviet television coverage. The result is an authoritative study that is highly recommended for both specialists and general readers.

GORDON B. SMITH

University of South Carolina

Eastern Christianity and Politics in the Twentieth Century. Edited by Pedro Ramet. Durham: Duke University Press, 1988. 471p. $47.50.

The central focus of this volume of essays, the first of three on "Christianity under stress," is the complex set of historical relationships between the Orthodox Church and the secular state—principally those of Eastern Europe. This entails a concern with events stretching back to the Council of Chalcedon in A.D. 451, although main emphasis is given to the last hundred years. It also entails a fairly detailed consideration of the particular events and personalities featured in the histories of the now 22 autonomous (or "autocephalous") churches of the Orthodox community. The analytic framework that is intended to hold these church-by-church case studies together is provided in a three-chapter introduction. In the first, the editor introduces the central themes: nationalism, in which the churches are examined as institutional symbols of national identity and carriers of national culture; cooptation whereby the state seeks to exert control over the church; and opposition, which includes both internal dissent within the church and ecclesiastical hostility towards state policies. These are then woven into a typology of basic church-state configurations. Russia, for example, is placed in the category "simple co-optive-antinationalist" (pp. 18–19). There are, then, in further introductory chapters, a review of Orthodox theology and a very useful overview of the history of church-state relations in the Orthodox world. The remaining 16 contributions then examine these relations on a case-by-case basis, including even the Orthodox church in the United States, Finland, and Ethiopia.

It is, perhaps, apparent, therefore, that the book is truly comprehensive in its coverage. Every significant Orthodox church, and even those that have ceased officially to exist (Albania) or are numerically very small (Hungary) find their place. To this extent, the editor has brought together a range of authors, expertise, and materials not found elsewhere in the literature. There are indeed many volumes on religion and politics in specific East European countries, studies of particular Orthodox churches, and even general surveys of contemporary religious conditions throughout Eastern Europe. This befits the historical and even current importance of the subject. But none (to my knowledge) looks at orthodoxy and politics in quite this way.

However, the question perhaps needs to be asked how much analytic meaning this particular coverage conveys, focusing as it does on an ecclesiastical rather than an avowedly political category. It is clear, in this context, that autocephaly has been an important political issue in Orthodoxy, and one that is not found in quite the same way in the Western experience. At the same time, the distinctive political significance of the Orthodox experience is not always made clear. Thus, we learn a lot about the detailed events of particular church-state histories but much less about how

Orthodox theology and self-understandings informed and shaped those histories. To that extent, the potential of Orthodoxy as a coherent political as well as ecclesiastical category is not fully realized. Perhaps, for example, some chapters on how the Orthodox episcopate tend to view their role in relation to politics or how Orthodox forms of church government shape the particular form of political and social outreach or how the laity view the Orthodox Church as a political actor might have made the particular ramifications of Orthodoxy in relation to the world clearer. Such a thematic, and perhaps more comparative approach might then have also fleshed out the sense of variations on the basic Orthodox experience and given the reader a firmer and fuller grasp on the typology sketched in the introduction. As it is, and lacking any concluding chapter, these broader aspects are somewhat underplayed.

The individual chapters come from a wide array of contributors, including political scientists, ecclesiastics, and historians. All are both clear and informative, and the expert knowledge that they collectively bring to the volume is impressive. Furthermore, all but two of the pieces are new. Thus, we have a very solid review of the Russian Orthodox Church (ROC), although one that does not particularly break new bround. One possibly notable omission here, however, is any mention of the potential impact of *perestroika* and *glasnost.* It is clear that so far no qualitative or legal change has occurred in the ROC-state relationship. But there are signs that the potential for change is there and, indeed, has been detected by the church's leadership. Beyond Russia, there are some very interesting and useful essays, especially those on the Orthodox churches of Greece (by Theofanis Stavrou) and Romania (by Alan Scarfe). But some of the others seem to be rather routine historical treatments or rather narrow in focus—such as the interesting but rather odd chapter on Russian Orthodox Church publications. One particular essay that represents in some ways a missed opportunity is the chapter on the Orthodox Church in the United States. Its presence ensures the book's comprehensive coverage, and we learn a lot about its internal workings and development. But there is relatively little, if anything, on how it responds to the U.S. political environment and engages with, or fails to engage with, U.S. political and governmental institutions.

This volume is a mine of information about the Orthodox experience in a wide variety of historical contexts. But it does seem that its potential is not always realized in the particular contributions and the way those contributions make up the broader whole.

GEORGE MOYSER

University of Vermont

In Pursuit of Lakshmi: The Political Economy of the Indian State. By Lloyd I. and Susanne Hoeber Rudolph. Chicago: University of Chicago Press, 1987. 529p. $42.00 cloth, $16.95 paper.

This is a comprehensive study of India's political economy. By no means the first, it distinguishes itself from Francine Frankel's *India's Political Economy* (1978), Ron Herring's *Land to the Tiller* (1983), and others by examining the subject through a theoretical framework using the contrasting models of demand polity and command polity. The Rudolphs explain India's political economy by looking at the interaction since independence between increased activity in the sphere of political mobilization and the changing capabilities of the Indian state.

The demand polity model which treats the citizens as the sovereign consumers in a competitive market economy is used to assess whether the economic policies of the Indian government were a response to the people's demands as expressed in party competition in elections and through representation by interests, classes, communities, and movements.

In the command polity model it is the state that is sovereign. The model is used to ascertain whether the economic policies adopted were a product of the preferences of the elected and appointed government officials who dictated the policies and also implemented them. What is revealed is that during certain periods the command model was dominant, while at other times the demand model played the primary role. A careful scrutiny of the arenas of politics and economy also reveals that dominance of the command or demand polity was closely related to the character of the leadership in power at a given time.

According to the authors, an examination of the role of the state in the context of command politics manifests that the state plays a dual role: "Even while retaining conventional state attributes and powers, the state as third actor has become a major component of the economy and of society. . . . The self determining state is in a position to serve itself and, like other self-interested actors, to be a source of exploitation or injustice" (p. 400).

The thorough and insightful scrutiny of India's political system and economy, based on extensive research and empirical data, leads the Rudolphs to observe that the strong points of the Indian state are its centrist pattern of partisan politics and its secular character. Secularization continues to be challenged. Religious rivalries surface often. But no strong national parties have emerged with appeals to the voters based on religious affiliation. Socialism, democracy, a mixed economy, and the foreign policy of nonalignment are features of the Indian state that are accepted by all the segments of the political community. India is a pluralist society politically and socially; class consciousness is missing in both spheres of activity. Organized workers and private financial and industrial capital, the two forces that could form the base for class politics, are politically marginal.

The authors also state that India as a case study reveals that there is no direct link between authoritarianism and economic growth. Contrary to the belief of some scholars and statesmen that authoritarian rule has been positively associated with economic growth, The Rudolphs maintain that available evidence indicates that "both good and bad economic performance occurred under both authoritarian and democratic regimes" (p. 223).

Significant attention is given to "students" as a political and economic force. It is pointed out that the pattern and capabilities of higher education in independent India have changed radically from the pattern that prevailed under the British, designed to turn out clerks and administrators to serve the masters. India's investment in higher education and research has given birth to a highly professional, technically expert and well-institutionalized bureaucracy that plays a vital role in economic development. On the other hand, unemployment among the educated is on the increase. Some three million college-educated unemployed constitute a force that influences the minds of college students and can channel them toward political movements of a highly disruptive nature.

A major portion of this book is devoted to an analysis of the politics and economy of the agricultural sector; for even to this day some 70% of the nation's labor force is employed in that sector and nearly 75% of the population continues to live in rural areas. The authors examine in detail the different strategies launched by the Nehru, Mrs. Gandhi, and Janata governments to solve the problems of the agricultural sector and to mobilize the support of the agricultural groups for government policies through elections.

The Rudolphs divide the agrarian producers into four agrarian classes: agricultural laborers, small holders, bullock capitalists, and large land-owners. As part of their analysis of the input of each of the classes in the area of politics and economy, they observe that evidence does not support the judgement of some Indian scholars that the spread of commercial agriculture was creating two classes: capitalist farmers and laborers. In their view, evidence suggests that capitalist farmers and wage laborers are neither the principal actors nor the only actors in the agricultural sector of the economy. Independent cultivators using their own and household labor (bullock capitalists) will remain the dominant political and economic force in the agricultural community. However, the appearance of prosperity in the agricultural sector has not eliminated the problem of the rural poor. The authors identify the rural poor and explain why they are not in a position to become a strong political force. And while the issue of poverty has been raised in every election, at the all-India level poverty in general remains as widespread as it was at the time of independence.

This book is an important contribution to a substantive understanding of the relationship between India's economy and politics. Written in clear and precise language with substantial information and in-depth analysis, it is a valuable guide for all those specializing in India's politics and economy.

SUDERSHAN CHAWLA

California State University, Long Beach

Rationality and Revolution. Edited by Michael Taylor. New York: Cambridge University Press, 1987. 271p. $37.50.

As Michael Taylor explains in the introduction, the essays in this volume each seek to show "that there *is* a useful role for the ideas and theories of rational choice in the study of revolution and rebellion" (p. 1). The volume as a whole is intended to challenge the structural accounts of revolution advanced by macrohistorical theorists. The contributors mount this challenge "by showing rational choice theorizing in action" (p. 2) rather than by engaging in abstract methodological disputes.

The book is divided into two parts of four essays each. Part 1 focuses on preindustrial societies. Samuel Popkin discusses political entrepreneurship among the Vietnamese peasantry. Michael Taylor and Craig Calhoun each explore the ways community can ground rational participation in revolutionary collective action. And James Tong offers a detailed empirical study of rebellion during China's Ming Dynasty. Part 2 is more abstractly theoretical. Michael Wallerstein and Adam Przeworski probe the strategic situation of workers in advanced capitalism. Jon Elster assesses Marx's views on the capitalist state and on transitions between modes of production. John Roemer presents a rational choice account of revolutionary ideology. The final contribution is a reprint of Raymond Boudon's essay, "The Logic of Relative Frustration."

Many readers will realize from this brief overview that the contributors tend to refine or extend previously published work. The essays here, like the earlier work from which they stem, are of uniformly high quality. The obvious question is why the book was issued only in an expensive cloth edition. But the character of the contributions also allows me to devote the remainder of my review to the two that I find most interesting.

Michael Taylor's "Rationality and Revolutionary Collective Action" is the centerpiece of part 1. Taylor directly criticizes the structural, antivoluntarist methodology advocated in many macrohistorical studies of revolution. He effectively shows how in practice, despite their methodological pronouncements, macrohistorical theorists are unable to eliminate intentional factors from their work. And he suggests how they might produce more-finegrained accounts of revolution by abandoning their misconceptions about rational choice and other forms of intentional explanation.

Specifically, Taylor shows how various macrohistorians correctly cite the role of community in grounding successful peasant rebellions. Yet, he complains, "the historians of these rebellions have had a lot to say about community but nothing to say about rationality" (p. 81). He shows how community provides the mutual knowledge and potential sanctions that make participation in rebellious collective action individually rational. Starting from a "thin" (p. 66) conception of rational action (e.g., instrumental pursuit of self-interest) Taylor connects community and rationality in compelling ways. He also chides macrohistorians for thinking that intentional explanation cannot account for unintended outcomes. Such a view clearly ignores the preoccupation rational choice theorists have with the unintended results of interdependent action.

As Taylor readily concedes "there are facts about collective action that the thin theory is unable to account for" (p. 64). And John Roemer's "Rationalizing Revolutionary Ideology: A Tale of Lenin and the Tsar" addresses precisely one of the weak points of rational choice theory. For Roemer our primary task in studying revolutions is to identify how participants overcome "the collective action problem" (p. 229). He reduces this task to explaining how the preferences of potentially revolutionary constituencies can be transformed from those typical of a prisoner's dilemma into those typical of an assurance game. Somewhat surprisingly, then, his essay on revolutionary ideology explicitly "sidesteps" (p. 230) this issue. Roemer views ideology not as a way of transforming individual preferences but "as a self-imposed limitation placed on one's feasible set" (p. 230). He then asks whether we can "explain why people have the ideologies they do" (p. 230).

Roemer models a two-person game between a revolutionary leader ("Lenin") and the authorities ("the Tsar") who respectively seek to mobilize and to block a potentially successful revolutionary coalition. He shows that under certain minimal, plausible conditions "it is just good strategy" (p. 239) for the Tsar to impose relatively more severe penalties on the poor than on the rich. Likewise, "it is just good strategy" (p. 239) for Lenin to espouse progressive redistributive policies. Roemer explains the actions of Lenin and the Tsar without recourse to claims about their prior ideological commitments. He literally rationalizes ideology.

This result seems to startle Roemer. He protests that the moral of his tale is not the "cynical" one that "people adopt the ideologies they do in order to be successful" (p. 244). Rather, only those political leaders who actually adopt "efficient" ideologies will survive the process of political "natural selection" (p. 244). Unfortunately, such evolutionary metaphors, which preclude intentional choice, obscure rather than answer Roemer's initial question. He has shown that many of the actions of political leaders might be explained without reference to ideology—as simply "good strategy." To avoid drawing a cynical lesson from his tale we must explain the independent force of ideology in political action. Roemer provides no such explanation. But he compels us to confront the task squarely.

JAMES JOHNSON

University of Chicago

Socialist Authority: The Hungarian Experience. By Peter A. Toma. New York: Praeger, 1988. 289p. $49.95.

Authority is an incidental theme of Peter Toma's study of contemporary Hungarian society. This detailed, empirically grounded study is preoccupied instead with societal attitudes and behavioral pathologies, and its chief strength lies in what we learn of the activities of "*homo oeconomicus*" under "goulash communism." Along the way we are made to appreciate the ingenuity of the Hungarian nation in pioneering a modern, instrumental type of socialist system that great Communist powers like China and the Soviet Union have struggled to imitate, as well as the ingenuity in finding subtle ways to circumvent the rules of the reformed economic system.

According to Toma, the study seeks to demonstrate "the shrewdness of both the rulers and the ruled" (p. xv), and this he does well. He describes what represents rational economic behavior in the context of the functioning of three different economies: the traditional state- and cooperative-managed one; the market-shaped one; and a third designed to manipulate the opportunities (such as shortages of goods or inadequacy of services) provided by the first two. The result of such a system, as Hungarians put it, is that "if you work, you don't have time to make money!" (p. 140); another view is that "everyone is building his own socialism" (p. 135).

Western economists and political scientists have rightly focused on the 1968 New Economic Mechanism (NEM) and subsequent refinements to it as the explanatory source for the configuration of Hungarian society today. By contrast, the social consequences of NEM have been carefully studied by Hungarian sociologists but, with a few notable exceptions, have largely escaped the attention of their Western counterparts. Toma's volume helps plug this gap. The participation of most Hungarians, in one form or another, in the third economy has produced both the "new type of personality" and the "new sorts of inequality," both the "new Hungarian culture" and the "new social character" (grounded, ironically, in many old traditions), that go beyond economic or political issues. In this volume, the pathological side to the new Hungary is especially well documented in areas ranging from the homebuilding industry and academic life to marriage, suicide, and soccer.

Some of the author's assertions are self-evident, banal, or overly generalized. For example, with regard to sexual relations, Toma's investigation finds that "parties are frequent meeting places—they are unsupervised and often end in lovemaking" (p. 169). This chapter on the transformation of the family and of religion would be strengthened if prevailing attitudes in Hungary were linked to a more universal pattern: apart from behavior at parties, population decline would then also be seen as a phenomenon not unique to Hungary. On the issue of inequalities, Toma, in spite of disaggregated and complex statistics on income differentials (Table 7.3), simplistically observes, "There is an economic stratum of society (ca. 30 percent) that is becoming steadily poorer; another 30–40 percent that maintains its standard of living; and finally about 30–40 percent of the population, whose life is becoming steadily better" (p. 141). Hungarians' perceptions of inequalities are self-serving and no less enlightening, however; an overwhelming majority agreed that some Hungarians were too well off, but no consensus existed as to whether some Hungarians were not well enough off (p. 218).

Toma's use of primary materials is impressive and he offers the non-Magyar-speaking East European scholar access to a broad array of official discourse, editorial polemics, and opinion survey results not otherwise available. A price Toma pays for his occasional overpreoccupation with detail is theoretical fuzziness. Though he provides incisive accounts of the concepts of socialist authority in chapter 1 and social character in chapter 7, it may have proved more useful to have reversed the order with which these were treated; for it is the notion of social character that forms the central theme and is empirically operationalized in the study. Another consequence of training on Hungarian source material is uneven treatment of secondary sources; research carried out by Walter Connor, Albert Simkus, and Ivan Szelenyi is not referred to, though much of it touches on Toma's problematic.

For an understanding of the mayhem prevalent in Hungarian economic and social life today, *Socialist Authority* serves as a colorful introduction.

RAYMOND TARAS

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Judicial Selection: The Cross-Evolution of French and American Practices. By Mary L. Volcansek and Jacqueline Lucienne Lafon. Westport: Greenwood, 1987. 168p. $35.00.

This small volume offers a number of exciting features and also a few frustrating ones.

The book is the only comprehensive cross-national comparative analysis of judicial selection presently available. The comparison is not only across two political cultures but also through time within each country. The two authors are among the leading scholars on the judiciary in their respective countries. The chapters on France are the most comprehensive historical summary available in English and the review of the U.S. experience does an admirable job of synthesizing the selection process in the complicated dual system.

Studies of judicial selection have almost always been comparative. Ordinarily the methods of selection are regarded as causal factors. Another approach is to recognize differences between political cultures and to use these differences to explain variation in selection. Political cultures are the causal factors. In both approaches the ultimate goal, rarely achieved, is to find explanations for why judges behave in particular ways. Volcansek and Lafon have taken the more comprehensive and difficult comparative cultures approach.

Assuming that judges are political agents, whether in the United States or France, they achieve their places on the bench through a common recruitment process. Following the work of Seligman and colleagues' *Patterns of Recruitment* (1974), Volcansek and Lafon argue that judges are selected by a process from certification (Who is eligible?) through selection (What are the informal qualifications and who are the sponsors?) to role assignment (Are the judges accountable or independent?). This structure of recruitment is the common denominator for the cross-national analysis, but the respective political cultures give often contrasting content to that structure. Thus, the authors assess the beliefs, feelings, and values concerning judicial selection held by the political elites in order to account for the "shifts and changes in the certification and selection phases of judicial recruitment in both nations" (p. 3).

Under the *ancien regime* France experienced a system of venality whereby judicial positions were purchased and often inherited. Thus, money and position controlled certification and selection. However, the judges enjoyed considerable independence from royal intervention. Following the revolution, elections were instituted with jurisdictional limits placed on the judges that robbed them of power and independence. Napoleon inaugurated executive appointments and instilled more professionalism in the judiciary by insisting on a law degree or a judicial apprenticeship from judicial candidates. Later, because of the suspicions surrounding law degrees, qualifying exams were included. Finally, judges are now subject to testing, schooling, and promotional processes as members of the civil service. Although executive control has remained since Napoleon, the judges have gained security and some independence through the civil service and perhaps through their own "union," the Syndicat de la Magistrature.

Across the Atlantic, the colonial experience taught U.S. citizens to hold executive appointments suspect. Consequently, state judges were recruited by legislative selection or some combination of executive appointment tempered by legislative approval. At the federal level the English liberal idea of republicanism and separation of powers dictated an independent judiciary, appointed by the executive but in practice with unlimited tenure once approved by the Senate. The influences of the French Revolution, the arrogance of the Federalists, the popularity of Jeffersonian politics, the individualism of the western frontier and the populism of the Jacksonian revolution all combined to provide for popular elections of state judges largely replacing the appointive-legislative recruitment of the earlier period. State judges became more accountable. Finally, as the legal profession grew stronger and worries of too much partisanship entered the recruitment debate, various merit systems—Missouri plans—became popular.

The authors conclude that "attempts to balance the requirements of ideology, politics, and legal competency characterize" the recruitment in each country (p. 141). Certification in both countries shows a "middle or elite" class bias (p. 141) and "proper political persuasion" separates the eligibles from those ignored by the recruitment process (p. 142). The selection phase in the United States gives considerable weight to political party considerations, and in France, although the civil service has eliminated sponsorship, life style and fashion (p. 143) seem to be important selection criteria.

Despite the comprehensive accounts of the two selection systems, the volume has some frustrating features. Although the work is cross-national, the comparative framework is not strictly adhered to throughout. Only the introductory and concluding chapters attempt to pull together the rich details of the intervening descriptive sections. Consequently, the chapters on French recruitment lack any synthesis that would help to make some general sense out of the details. The role the organized bar plays in both countries and the importance of judicial review in the United States are largely ignored, although they undoubtedly have shaped judicial selection. As with almost all judicial recruitment studies, the authors fail to speculate on how selection influences the behavior of judges.

The book is clearly a benchmark and should inspire more comparative studies of the judiciary. As the authors have shown, judges are indeed products of political cultures, but the jurists also have had an impact on the culture of which they are a part. I hope the authors cooperate again on a study of how the products of particular recruitment schemes go about their judging.

CHARLES H. SHELDON

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Uruguay: Democracy at the Crossroads. By Martin Weinstein. Boulder: Westview, 1988. 160p. $29.95.

As the smallest nation in South America, Uruguay has historically been dwarfed by neighboring Argentina and Brazil and has never earned significant scholarly attention abroad. Yet as the founder of the modern Uruguayan welfare state, Jose Batlle y Ordoñez, said, "We may be a poor and obscure mini-republic, but we . . . have advanced mini-laws" (p. 25). By the late 1940s, Uruguay was no longer poor, having achieved through its meat and wool exports perhaps Latin America's highest standard of living. Its educational and nutritional levels not only ranked with Argentina as the highest in the region but were comparable to those of Western Europe. Batlle and his Montevideo-based Colorado party created an alliance between the nation's urban middle and working classes that served as the basis for political stability, a comparatively high level of pluralist democracy, an extensive welfare system featuring generous pension programs, and an income distribution that, though still marked by sharp urban-rural gaps and intense rural poverty, was among the most equitable in Latin America.

Yet the period following the Korean War was marked by prolonged economic stagnation, increased class conflict (led by a militant, Marxist-dominated trade union movement), increasing alienation of intellectuals and much of the political class, the rise (in the 1970s) of the Tupamaro urban guerrilla movement, and the gradual collapse of democracy. The military-dominated regime that took power in 1973 (the same year the Chilean military toppled Salvador Allende) transformed the country, once known as "the Switzerland of South America," into one of the region's most repressive bureaucratic-authoritarian states. Only recently, with the March 1985 inauguration of an elected civilian government, has Uruguay returned to democracy.

Martin Weinstein's book traces the rise of Batlle's welfare state, the rise and fall of Uruguay's democratic political system, the nature of the country's rightist dictatorship, and the prospects for its fragile restored democracy and its debt-ridden economy. The volume is part of a country study series published by Westview Press. One of the important contributions of that series is that it provides general studies, most of them suitable for classroom use, of many of the region's smaller, less-well-known nations. In examining Uruguay, Weinstein explicitly or implicitly raises issues that transcend that small nation's boundaries and are of importance to political scientists studying Latin America and the rest of the developing world. Given its relatively high educational level and political stability, why was Uruguay unable to sustain its standard of living? Uruguay's experience, like that of neighboring Argentina, may suggest to some scholars the inherent limits of growth within a dependent economy, while to others it may indicate a lack of entrepreneurial skills or national vision in the culture. How did Uruguay, like nearby Chile, move from being the region's most open democracy to being one of its most repressive dictatorships (with the highest percentage of political prisoners of any nation in the world, according to Amnesty International data)? In view of its large middle class (or, at least, its middle-class culture), its relatively equitable income distribution, and its extensive welfare system, why did Uruguay develop such bitter class conflict? How did a country with so little crime and political violence for most of the twentieth century spawn such a violent guerrilla movement? Finally, what lessons, if any, does Uruguay offer regarding the restoration and maintenance of democracy?

Weinstein notes that the nation's urban-oriented welfare state, inefficient import-substitution industrialization, and extensive state bureaucracy (serving as a vast employer for the large, educated, urban middle class) depended on extraction of export revenues from the rich livestock sector. Drawing on M. H. J. Finch's outstanding book, *A Political Economy of Uruguay since 1870* (1981), he cites the failure of the landed elite (who were spared land reform through a tacit understanding with Batlle's Colorados) to modernize and thereby remain internationally competitive. He also focuses, quite correctly, on the problems of the patronage-based political system and its failure to develop a national vision. Thus, the faction-ridden political parties, ranchers, urban business leaders, labor unions, Tupamaro guerrillas and the military all suffered from tunnel vision.

Weinstein feels that the restoration of democracy has meant a return to business as usual with few new solutions to the ongoing economic and political stalemate. He is pessimistic about the prospects for sustained democratic government or economic revitalization. His work is the first English language book on Uruguayan politics since the collapse of the dictatorship. It is well written and clearly argued (though at times repetitious). It is not, however, as theoretically rich as Charles Gillespie's essay in *Transitions from Authoritarian Rule: Latin America* (edited by O'Donnell, Schmitter, and Whitehead [1986]) or the essays by Gillespie, Juan Rial, and others in *Uruguay y la democracia* (edited by Gillespie and colleagues [1985]) or Rial's *Partidos politicos, democracia y autoritarismo* (1984). Ultimately, several questions remain: Could things have been different? Can Uruguay's economic and political systems still modernize? To what extent has the problem been one of dependency and to what extent has it reflected a failure within the political culture?

HOWARD HANDELMAN

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Pluralism, Corporatism, and Confucianism: Political Association and Conflict Regulation in the United States, Europe, and Taiwan. By Harmon Zeigler. Philadelphia: Temple University Press, 1988. 249p. $29.95.

An ample amount of recent work in comparative political economy has attempted to understand how various political institutions and processes affect the economic performance of capitalist economies. Part of this work has focused on the question of whether the neocorporatism of the smaller Western European nations promotes good economic performance relative to their more pluralistic counterparts. Others have focused on the characteristics of the political economies of democratic Japan and, to a lesser extent, of authoritarian Taiwan, South Korea, and Singapore for answers to this question. Harmon Zeigler's new book lies at the juncture of these two research foci.

In actuality, Zeigler is concerned with two somewhat disparate questions: Why do people join interest associations? and What difference does the mode of interest intermediation make for system performance? While the bulk of the book is focused on the latter question, Zeigler provides a limited yet interesting treatment of the relative importance of collective goods or "shared interest," a factor emphasized in traditional pluralist theory, and Olsonian selective incentives as forces shaping an individual's decision to join an interest group. Drawing on recent research and survey data analysis, Zeigler finds that membership is motivated by the desire for collective goods *and* the lure of selective incentives in both the individualistic United States and collectivist Taiwan. The relative importance of these forces is determined by what type of group and what stage of membership renewal one is focusing on; selective incentives, for instance, generally become relatively more important over time as inducements to sustain group membership.

However, despite the insights provided by this analysis, the typical reader might be disappointed by the fact that Zeigler does not extend the analysis to the West European pluralist democracies and neocorporatist systems or, in turn, explore the interrelationships between group origins and maintenance and the system of interest representation. More centrally, one might ask whether Zeigler's examination of the effects of the mode of conflict regulation on system performance would have been significantly enhanced by inclusion of a more systematic analysis of the economic, political, social, and cultural forces—structural and episodic—that shape the development of systems of interest representation. Indeed, a fair amount of relevant work on which to build has appeared recently, most notably Peter Katzenstein's *Small States in World Markets* (1985). Nonetheless, Zeigler is seemingly content to jump somewhat abruptly from an analysis of the origins and maintenance of interest associations to an examination of the importance of the mode of interest intermediation for system performance.

With respect to this second question, Zeigler's analysis hinges on a synthetic conception of corporatism; that is, for Zeigler, corporatism is a system of interest intermediation characterized by functionally differentiated, state-licensed interest associations (typically but not necessarily peak associations), a relatively high degree of state-group concertation in the area of economic management, and a collectivist, consensus-promoting cultural environment. Pluralist political economies are characterized by heterogeneous, thoroughly autonomous and volunteeristic interest groups; low levels of state-group concertation; and an individualistic, competitive political culture. According to Zeigler (p. 18), "The distinction between societal and state corporatism is based largely on the degree of dependency. In societal corporatism, interest groups are autonomous. . . . In state corporatism, interest groups are dependent, auxiliary organizations."

Generally, such an overarching conceptualization has distinct advantages and disadvantages. One important advantage is the ability to compare diverse political economies. Indeed, Zeigler, in three brief chapters, provides an insightful discussion of Taiwanese (and other Asian) political-economic interaction. While distinguishing the Asian state corporatist systems on the basis of their authoritarian bent, the impotence of labor, the reliance on representational mass parties, and the fundamental importance of a Confucian ethical system, Zeigler is nevertheless able to draw interesting parallels between these nations and the societal corporatist states of Western Europe on the dimensions of state-group concertation and patterns of policy making.

Unfortunately, a broad, synthetic understanding of corporatism may also lead to certain theoretical and empirical problems. These difficulties become evident in Zeigler's systematic empirical analysis of the relationship between the degree of societal corporatism and facets of economic performance and political stability. Consistent with his theoretical framework, Zeigler employs a measure of societal corporatism that ranks Japan and Switzerland equally with Sweden, Norway, and Denmark. The author's subsequent empirical analyses reveal that when controlling for "common correlates" of neocorporatism—the ideological orientation of the party system, the scope of unionization, and wealth—neocorporatism is positively associated with low unemployment, low inflation, and low levels of political instability; is not related to economic growth rates and income inequality.

However, Zeigler's results may be partially conditioned by a lack of sensitivity to the theoretical bases for many of the original claims for neocorporatist effects on system performance and by his particular operationalization of societal corporatism. Specifically, much of the theory that underpins the claims that societal corporatism promotes growth, employment, low inflation, income redistribution, and political stability emphasizes the role of an encompassing, highly centralized labor organization that is incorporated in tripartite networks of national economic (and social) policy making; that is, unlike Zeigler's conceptualization, this body of work designates the existence of relatively inclusive, peak associations of both business and labor as a fundamental element of societal corporatism. Thus, operationalizations of this understanding of corporatism, as well as variants that emphasize the joint presence of a strong, centralized labor movement and a labor-oriented government would rank Japan and Switzerland—countries in which labor is relatively weak and marginal to networks of policy concertation—much lower. Indeed, a fair amount of recent work employing this latter conceptualization of corporatism and its associated operationalization suggests that the presence of societal corporatism and strong labor-oriented governments may be conducive to income redistribution and economic growth. (See, for instance, the excellent analyses of the political determinants of economic growth rates in recent articles by Lange and Garrett and by Hicks, in *Journal of Politics* 47 and 50 [1985 and 1988], respectively.) Generally, one might suggest that more nuanced and sophisticated theoretical differentiation, measurement, and model specification would have, at minimum, produced more compelling findings irrespective of the direction of the results.

Overall, despite the aforementioned difficulties, Zeigler's book may be recommended to students of comparative politics and political economy. The scope of topics covered, the breadth of comparison, and the occasionally insightful and suggestive analysis make for generally engaging reading.

DUANE H. SWANK

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Cuban Political Economy: Controversies in Cubanology. Edited by Andrew Zimbalist. Boulder: Westview, 1988. 240p. $42.50.

This sustained critique of prior research on Cuba since the revolution devotes about a third of its attention each to economics, sociology, and political science. Much of the emphasis throughout the book is on questions of theory and method. The first four chapters pay special attention to the macroeconomics of growth, distribution, and planning. Two chapters focus on statistical and related topics in the assessment of Cuba's performance in the areas of health, education, housing, nutrition, and employment. One chapter focuses on the study of women in Cuba, and three on the nature of studies about Cuban domestic and international politics. There is also an excellent chapter by Susan Eckstein on the reasons for "Cuban internationalism," which is the only one in the book to focus on the substance of a topic and not on the prior scholarship about that topic.

At their best, the authors pose important questions that any field of scholarship must address. These include the personal and ideological inclinations of scholars prior to and during their study of a subject; the overarching paradigms within which research has been undertaken; the appropriateness and utility of the theoretical frameworks employed; the logical standing and empirical validity of interpretations; and more particular points of analysis. Many of their criticisms of the work of other scholars are thoughtful and warranted and should make for better scholarship in the future.

On the other hand, several of the essays at times suffer from what comes across as personal hostility toward those whose work is being criticized; of blanket categorization of individuals and groups of scholars as agents of the U.S. government; of an inability to consider that some of the scholarship of those whose work is under criticism might still have worthwhile features; of ideological intolerance toward those of other views; and of serious inaccuracies or misleading renditions of aspects of the work of scholars whose research is under scrutiny.

This is especially regrettable because many of the authors, including the editor, have in other circumstances produced first-rate research that has enhanced scholarly understanding of contemporary Cuba; they have also been constructive colleagues in a community of scholars.

From the perspective of reviews of the state of scholarship, the main weakness of the book is that most of the authors fail to see variation among those scholars with whose works they disagree. One approach is to identify all other scholarship as constituting a single school of thought (evident in most of the chapters on economics and in chapter 10 on politics). Another approach is to acknowledge some differences among scholars whose work is under review but to insist that these differences do not matter very much (pp. 138, 222). In general, this book's authors portray the world of scholars dichotomously: there are the good and the bad, and the bad—none in this book—are very bad.

It is noteworthy that one of the rare recognitions of some variation among scholars in what this book calls "mainstream cubanology" comes from José Luis Rodríguez (pp. 28–31), the only author in this book who lives and works in Cuba. Susan Eckstein's chapter sets yet another model to emulate. While she is clearly critical of some of the works being criticized by her coauthors in this book, she shows how a responsible scholar can integrate her own research with that of others to yield thoughtful conclusions. Let them serve as examples for others who may write about these topics in the future.

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INTERNATIONAL RELATIONS

Western Europe in Kissinger's Global Strategy. By Argyris G. Andrianopoulos. New York: St. Martin's Press, 1988. 262p. $47.50.

A conservative reform of the international system was the overriding objective of the Nixon-Kissinger foreign policy strategy. The Nixon-Kissinger strategy required the transformation of the Soviet Union and the People's Republic of China into status quo powers, the stabilization of the superpower balance of military power, and the creation of an agreed-upon set of rules governing superpower conduct. The intended result was a stable structure of peace that would allow the United States to make an orderly retreat from empire without jeopardizing its core interests in Western Europe, Japan, and the Middle East and without sacrificing its interests along the periphery. During the Nixon-Kissinger tenure, relations with our Western European allies fluctuated between petulance and a cold embrace. The attributed causes of Atlantic discord at this critical juncture in U.S. foreign policy range from Kissinger's alleged duplicity in his dealings with the Europeans to the largely irreconcilable geopolitical and geoeconomic differences of interest between the United States and its Western European allies.

It is Argyris G. Andrianopoulos' thesis that the conflicts between the United States and Western Europe during the period 1969–73 can be attributed to the Europeans' "misinterpretation and/or disregard of [Kissinger] writings" (p. 6). Andrianopoulos correctly argues that Kissinger's fundamental foreign policy objective was the creation of a stable structure of power ensnaring the Soviet Union and the People's Republic in an international system of U.S. design; and that the United States would, when necessary, subordinate the interests of its Western European allies to that objective. To argue that the subordination or violation of Western European interests could have been mitigated by a careful reading—or even of Andrianopoulos' reading—of Kissinger's academic writings not only defies common sense but violates the expectations of *Staatsraison*—an idea central to the common conceptual framework shared by Kissinger and his European counterparts.

Moreover, I believe that Andrianopoulos errs in arguing that there is a consistency between Kissinger's academic writings and the Nixon-Kissinger foreign policy. Although there is a broad consistency between the Nixon-Kissinger foreign policy and Kissinger's understanding of international politics as articulated in *A World Restored* (1973), there are glaring inconsistencies between the foreign policies of the Nixon administration and Kissinger's works on the Atlantic Alliance. This disjuncture between theory and practice is found in arguments made (and consequently forgotten) by Kissinger in *A Troubled Partnership* (1965)—a critical text for Andrianopoulos—and in Kissinger's outdated view of international politics.

In *A Troubled Partnership*, Kissinger argued that the "deepest cause" of misunderstandings in the Atlantic Alliance was the difference between the European and U.S. historical perspective: a difference that has yielded incompatible statecrafts—in its conceptualization and execution—and has generated disparate foreign policy expectations on a broad range of issues. Yet on becoming national security advisor, Kissinger largely forgot his own counsel. Kissinger warned as well against U.S. unilateralism because it would only guarantee disarray in the alliance. While it is true that the compulsions of power forced the Nixon administration to act unilaterally (e.g., the confrontation with the Soviet Union during the Yom Kippur War), it is also the case that U.S. unilateralism threatened the economic security of its European allies. Kissinger also believed that a cohesive Atlantic policy required the United States to yield, eventually, a measure of autonomy in its foreign policy. Yet on access to the corridors of power, Kissinger proved as willing as his predecessors to guard U.S. foreign policy prerogatives against allied encroachments in the Atlantic area. Furthermore, Kissinger's call in 1965 for a cohesive Atlantic policy became, after 1969, an effort in the Nixon Doctrine to retain a lost U.S. hegemony by shifting the costs of empire onto the United States' dependents in the Third World and its European praetorian guard.

Perhaps most importantly, Kissinger's notion of an international system responding reflexively to the dictates of the realpolitik had long since lost its validity by 1969. At least two international systems occupied the same temporal space, one driven by the compulsions of military power and the other driven by the logic of economics; and both systems required complementary foreign policies responding to the demands of the other. The international system driven by military power had lost a great deal of salience for the problems facing the Western Alliance during the tenures of Nixon and Ford. The critical problem facing the Atlantic Alliance in the 1970s was the emergence of previously substrated geoeconomic tensions that were no longer held in check by an unquestioned U.S. hegemony. Henry Kissinger's intellectual compass initially excluded economic conflict as a fundamental source of Atlantic disrepair; rather, economic side-payments were the coin in which the Europeans would be compensated for U.S. supremacy in Atlantic security policy. Kissinger relied upon Europe's security dependency as the guarantee of Atlantic cohesion; but as he found out after the oil crisis, issues of economy could no longer be subordinated or sacrificed for issues of military security.

The book has a number of other problems: Kissinger's memoirs serve as Andrianopoulos' authority on the Nixon administration's foreign policy, with respect not only to Kissinger's motivations and intentions but also to those of his domestic peers and foreign counterparts; reference is repeatedly made to Kissinger's critics on specific policy issues without identifying them; the text is replete with punctuation errors and bothersome shifts of verb tense; and the Hallstein Doctrine is referred to as the "Holstein" Doctrine (p. 236). Moreover, Andrianopoulos' savaging of Jeffery Bendel's doctoral dissertation (on the same subject) was distasteful and unwarranted. Bendel reached conclusions that vary with those of Andrianopoulos. But had the latter examined Kissinger's dealings with the Western Europeans for the period 1969–76 rather than merely the first Nixon administration, Bendel's conclusions would appear, at a minimum, plausible. Andrianopoulos' assessment of Bendel's dissertation can be fittingly applied to his own book: "It cannot be used as a reference source because of its inaccurate and uncritical treatment of his sources" (p. 27).

JAMES CLYDE SPERLING

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Protectionism. By Jagdish Bhagwati. Cambridge: MIT Press, 1988. 147p. $16.95.

Resisting Protectionism: Global Industries and the Politics of International Trade. By Helen V. Milner. Princeton: Princeton University Press, 1988. 329p. $29.50.

A central puzzle in international political economy is why protectionism so often raises its ugly head: since the economic benefits of free trade outweigh the gains from protection, political scientists and economists have long explored why and how the political process subverts economic efficiency. We often read that declining U.S. hegemony, rising import penetration, and difficulties in industrial adjustments are leading the world down a slippery slope of protection. Evidence of this gloomy view is supported by the growing number of protectionist petitions since the early 1970s and the apparent rise in trade restrictions. Even though tariffs have continued to fall, the United States and Europe have almost doubled the amount of imports restricted with nontariff barriers since 1981.

Rather than follow the intellectual pack with this line of reasoning, the two books under review pose a more interesting question for the 1990s: since the incentives for protectionism seem so pervasive today, why do we have so much free trade? Bhagwati and Milner suggest that the rise in protectionism in the 1970s and 1980s has been overstated and, more important, that protectionism may not be as politically attractive as most scholars fear. Although their arguments and methodologies are very different, both books are essential reading for international political economy scholars because of their insightful identification of the political and economic incentives that promote freer trade.

The central theme of Bhagwati's short book is that the interplay of ideas, interests, and institutions have restructured the balance of forces between protectionism and freer trade. Bhagwati describes how economists have demonstrated with increasing certainty that free trade is good for *all* nations as well as individual countries. Not just the standard message of the benefits of freer trade, Bhagwati details the economic holes in the case for protection (infant industry and optimal tariff arguments), which he claims bolsters opposition to trade barriers. (Bhagwati, a master of coining phrases, calls this the Dracula effect—expose an evil to sunlight and you help to destroy it.) The bankruptcy of postwar import substitution strategies by developing countries further reduced protectionism's appeal.

Bhagwati correctly notes, however, that ideology alone was insufficient to explain the persistence of liberal trade. The power of ideas was complemented by a growing configuration of interests opposed to protectionism, which included the United States (because liberal trade promoted U.S. security interests), other industrial nations (because economies became more dependent on exports, even as world trade slowed), and especially multinational corporations (because their global operations demanded open borders). These various interests were reinforced by U.S. political institutions. Although the Congress has leaned toward protectionism, the executive branch has demonstrated that it can "harness" pro-export interests and bolster the antiprotectionist coalition.

This book's strength is its simple, yet powerful statement of the multifaceted constraints that reduce the likelihood of a new Smoot-Hawley. A weakness is that Bhagwati's personal commitment to the ideology of free trade may have blinded him to countervailing forces. For instance, if the ideology of free trade is so strong, why did the United States and European Community remain so protectionist in agriculture for the last 40 years? It is odd that the force of ideas would have so much power in manufacturing trade and so little in food. In addition, Bhagwati dismissed the new international trade theory, which is finding persuasive arguments in favor of protectionism. He does not reject them because they are analytically weak (the theories are based on a sophisticated argument about the role of market imperfection); rather he "opposes" them because informational problems make them politically difficult to implement (p. 109). Yet the mere fact that it's hard to do efficiently does not mean that a political entrepreneur (e.g., Richard Gephart) will not find a way to exploit the new theories. One who had used Bhagwati's logic 150 years ago would surely have dismissed Ricardo's theory of comparative advantage because of the political impossibility of moving from mercantilism to freer trade.

A second weakness in the book is that the author distrusts "anecdotal evidence" and dismisses counterarguments based on case studies; at the same time, he uses anecdotes extensively, often with misleading results. (For example, as evidence that multinational corporations (MNCs) are committed to free trade he cites a case where Motorola, an MNC, did not support an industry dumping petition [p. 77]. Yet Motorola was the *most* protectionist company in the industry on virtually every trade issue—it failed to promote the particular petition because it did not make the product!)

Bhagwati's weakness is Milner's strength. While Bhagwati's book has analytical breadth and no empirical depth, Milner has focused on one piece of the protectionist puzzle—the interests of firms in international trade—and written a definitive empirical study on corporate trade preferences. Milner has four straightforward hypotheses: during periods of economic distress and rising imports, (1) firms that are neither multinational nor export-dependent are most likely to advocate global protection; (2) firms that export heavily and maintain extensive overseas operations will be most committed to free trade; (3) high multinationality and low export dependence will lead firms to demand selective protectionism; and (4) firms that are dependent on exports but not multinational will also have mixed incentives but will usually promote freer trade.

To test these hypotheses, Milner devised a clever research design: she looked at six U.S. industries who suffered dramatic inceases in import competition in the 1920s and six in the 1970s and then compared those results with a similar set of six cases for France in the 1970s. Since the 1920s and 1970s had many comparable macroeconomic characteristics, this allowed her to test the impact of multinationality. Also, by comparing two different political systems like France and the United States, she partially controlled for the influence of domestic institutions. Her results were generally consistent with her hypotheses, finding that pressure for protectionism was intense in the 1920s because firms were largely domestic; but demand for protection was weaker in the 1970s in both the United States *and* France partly because the presence of MNCs led industries to be politically divided.

Milner's research also has some weaknesses: first, the theory is based on comparative statics —the hypotheses explain corporate preferences at a point in time, but they have no predictive power over time. Several of her "free traders" from the 1970s, for instance, quickly became protectionist after her research ended in the 1980s. Second, a few case studies are problematic. (Having studied some of her cases extensively, I had a very different interpretation of some results.) Nonetheless, Milner presented a sophisticated blend of international political economy theory and empirical research without overstating her case. While Bhagwati's *Protectionism* is an important review of the present state-of-the-art thinking in economics, Milner's *Resisting Protectionism* will be a more significant, longer-term contribution to the political economy of trade.

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Gorbachev's Russia and American Foreign Policy. Edited by Seweryn Bialer and Michael Mandelbaum. Boulder: Westview, 1988. 491p. $32.50 cloth, $15.95 paper.

The Soviet Union As an Asian Pacific Power: Implications of Gorbachev's 1968 Vladivostok Initiative. Edited by Ramesh Thakur and Carlyle A. Thayer. Boulder: Westview, 1988. 236p. $31.50.

That Mikhail Gorbachev has initiated a program of radical change of the Soviet political system is well established. What is not so clear is how far he can or will go and whether or not he will succeed. For the United States two additional questions are critical: What do Gorbachev's changes mean for U.S.-Soviet relations? And how should the United States respond to these efforts? Thirteen authorities on Soviet affairs attempt to answer these questions in the Bialer and Mandelbaum collection. Approximately half of the essays examine the historical background and current domestic conditions that have created what Timothy Colton calls the "crisis of coherence" and what Thane Gustafson calls a "crisis of survival." The second half of the book examines the implications of *glasnost* and *perestroika* for foreign policy with emphasis on relations with the United States.

No one can say, nor does any author here attempt to speculate, how radical Gorbachev will eventually become. There is a consensus that Gorbachev's reforms go beyond policy and organizational changes to include structural and institutional changes; yet none see his goals in such revolutionary terms as abandoning state ownership of productive capacity, a planned economy, or control of the political system by one party. One author quotes Gorbachev: "We are getting ready to strengthen socialism and not to replace it with a different system." As to Gorbachev's prospects for success the general assessment is that they are not good, certainly not in the short run (five years). Seweryn Bialer believes that Gorbachev's program will partly succeed and partly fail but will not bridge the transition to modernity.

There is no dissent from the view that Gorbachev's domestic and foreign policies serve not only the interest of the Soviet Union but that of the West as well and, accordingly, should be encouraged by the United States. However, as these analysts are not sanguine about Gorbachev's domestic success, neither do they see a quick end to the Soviet-U.S. competition in world politics. The cold war is not yet viewed as over, though different factors are stressed as the source of that conflict. John Gaddis sees the source in terms of fundamental conflicts of interest. Bialer argues that Moscow remains committed to the domination of Eastern Europe, a point stressed by William Leurs. Joseph Nye stresses the geopolitical roots of the cold war. Michael Mandelbaum believes that the cultural gaps are still too wide but that some future "internal transformation of the Soviet Union" could improve relations.

These essays originated in a series of workshops initiated by the East-West Forum, a New York based research and policy analysis organization. The author's conclusions, which are carefully qualified, reflect the informed and balanced judgment of a group of distinguished scholars, a diplomat, and a journalist. There is not a weak chapter in this collection. This volume will serve for some time to come as a fine introduction to the Gorbachev reforms and their impact on foreign policy.

The Thakur and Thayer book focuses on one aspect of Gorbachev's foreign policy, namely the initiatives toward Asia made in his speech at Vladivostok on 28 July 1986. In that speech Gorbachev proposed linking the economy of the Soviet Far East with that of the Pacific Basin states and introduced several initiatives that indicated a Soviet intention to play a more active role as an Asian Pacific power. He also made a strong bid to improve relations with China. The book is based on papers presented at an Australian Defense Force Academy symposium. The 11 authors are scholars and a few government personnel all of whom work in Australia or New Zealand. Each specialist examines the impact of the Vladivostok Initiative on the region or country of his or her specialty.

The authors differ in their assessment of the importance of the Vladivostok Initiative. Those writing about its effect on Soviet relations with India and Australia see virtually no change; those looking at Japan, the ASEAN countries, and the islands of the Southwest Pacific see very limited change. Those writing about China, however, believe that the Vladivostok Initiative has considerable significance for Sino-Soviet relations. Indeed several authors contend that China was the principal target of the Gorbachev speech. Gary Klintworth argues that the improving Sino-Soviet relations could "presage one of the most important new strategic developments in the Western Pacific region for the remainder of this century." The observations on China have in some measure been born out by subsequent events. On the other hand the chapter on Afghanistan fails to anticipate Moscow's policy changes in 1987 regarding the withdrawal of its forces. One idea stressed by several writers was the importance of Vietnam's occupation of Kampuchea as a barrier to normalization of relations between the Soviet Union and the countries of Southeast Asia. A major test of Soviet intentions was seen in its willingness to exert pressure on Vietnam to withdraw. With regard to Japan the critical barrier is Soviet occupation of the Northern

Islands. Paul Keal was not sanguine about Soviet concessions to Japan.

While some writers saw a substantial element of propaganda in the Gorbachev speech, there was a general sense that the Soviet venture should be viewed cautiously and hopefully and neither be uncritically embraced or rejected outright.

The quality of the papers varies. A few were short and superficial, some journalistic, and some scholarly. In general, the more substantial insights contained here concern the foreign policies of the targeted countries in the Pacific Basin rather than those of the Soviet Union.

JOSEPH L. NOGEE

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National Security Planning: Roosevelt through Reagan. By Michael M. Boll. Lexington: University Press of Kentucky, 1988. 271p. $26.00.

The Dangerous Doctrine: National Security and U.S. Foreign Policy. By Saul Landau. Boulder: Westview, 1988. 201p. $24.95 cloth, $9.95 paper.

Both of these books depict national security concerns as the key to postwar U.S. foreign policy, but that is about all they have in common. They adopt quite different perspectives; and neither provides, to my view, a sufficiently accurate analysis.

Michael Boll's book seems mistitled. It is not about national security "planning" as I would use the word. It is an account of the perspectives—the ideas and assumptions—that moved U.S. administrations in their pursuit of national security since the late 1930s. Perspectives are certainly part of planning but planning encompasses a good deal more that as the shortness of the book suggests, is not covered. What is included instead is a concise account of the policies that were adopted, plus descriptions at various points of the "threat" (Soviet military capabilities) those policies were meant to allay. This turns the book into a brief, nicely written overview—semiofficial in viewpoint—of what the United States has been up to under the rubric of national security since just before Pearl Harbor.

Given the author's intent, the length of the book, and the focus primarily on what policy makers assumed was true about the world, there is not a lot here that is new. The book contains a useful bibliographic essay, an effective account of the creation of the early national security establishment after the war, and a decent summary of the classic Soviet ideological approach to international politics. What's missing? Needed is a more insightful analysis of how the U.S. perception of the threat was, at various times, subject to serious distortions; and more pointed reminders that U.S. policy was often not the result of any serious "planning"; even suggestions that the intellectual framework of the day often functioned to rationalize policies derived from other motives, inclinations, and interests.

The Landau book is a leftist critique and does not notably depart from or improve on its predecessors in this genre. The image presented is of the imperialist United States, moved by its capitalist elite, managed by a national security state that has subverted democracy. This leads to conclusions that do not always fit coherently. For instance, the United States has been in charge of an empire since 1945, one ruthlessly sustained. The empire is now in the process of collapse, it seems; but if so, how ruthless can the United States have been? The national security state has totally manipulated public opinion, especially via secrecy. It has also now lost public support for any more Vietnam-style adventures. If so, what does this tell us about its effectiveness in manipulating public opinion? Capitalism in search of opportunities for exploitation is what U.S. foreign policy has been about. But then why has the nation run up such a huge trade deficit and such great debts while experiencing other reverses to the evident benefit of client states the U.S. was presumably exploiting? Surely an unusual form of exploitation.

Landau gives special attention at the end to the Oliver North fiasco. This is the national security apparatus gone wild, certain its goals should override every other consideration and secretly operating its own foreign policy. This is the national security perversion at its worst, eroding the foundations of our democracy. Actually, the North case is a striking example of how terribly frustrated the national security Neanderthals became as they discovered the limits on their power. Even with a totally sympathetic administration, long on public support, they could not just ignore the constraints imposed by the U.S. political system. Instead, they chafed at the unwillingness of the Congress, public opinion, our allies, and a host of interested groups to sustain their wilder ambitions and visions.

Landau seems to judge the system by whether some people in it are trying to do corrupt or evil things. But the system rests on the presumption that this is exactly what will happen. The way to judge is by whether such people can consistently succeed and whether what they do can either be hidden or successfully sold as praiseworthy. By this standard the U.S. system is probably much better than it used to be. Why else was North skulking around in secret? What happened to his operation once it became public?

Nothing appears here about the postwar Stalinist system that confronted Western nations and that surely helps explain some of the U.S. perspectives and behavior Landau condemns, at times quite rightly. Nothing is said about the fact that nearly everything the United States has consistently claimed was wrong with the USSR, in justification for the containment policy, is now being said (and acted upon) in the Soviet Union itself, usually at the highest level.

Many readers will share some of Landau's concerns, but what does he offer as solutions? A more alert Congress; revived activism by a coalition of liberals, the Left, and libertarians; and a new effort to make human rights the focus of foreign policy. None are compelling, in view of past experience.

The U.S. mania about national security and the creation of the national security establishment was a response to a real evil that made the "threat" seem all too obvious. But that doesn't excuse the U.S. failure to see the threat clearly for what is was, both at the beginning and later on; to better detect its alteration over time; and to remain in touch with the nation's traditions and historical perspectives in shaping policies to deal with it. Those failures meant that U.S. national security policy frequently came to grief, largely because it was often absurd. Boll's book does not fully tell us this was so, and Landau's book does not fully tell us why.

PATRICK M. MORGAN

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Power in the Isthmus: A Political History of Modern Central America. By James Dunkerley. New York: Routledge, Chapman & Hall, 1988. 691p. $37.50.

Dunkerley has attempted an extremely ambitious political history of Central America since its independence in 1821. The lengthy, largely synthetic work draws heavily on recent English and Spanish sources, including some of the best scholarship from the isthmus. Dunkerley—also author of the respected *Long War* (1982)—writes from the perspective of the European scholarly Left.

Dunkerley devotes early chapters to the formation of Central America's liberal regimes, control by the oligarchy (1910–30), and the rise and consolidation of military rule (1930–60). He focuses on how socioeconomic forces have shaped isthmian political systems, successfully delineating critical differences among Central American regimes. Chapter 5 explores the 1950–80 Central American economic boom. He then spends two chapters on the pivotal Nicaraguan revolution and one chapter each on El Salvador, Guatemala, Honduras, and Costa Rica. There is a valuable selected bibliography.

I believe that this book has value for nonspecialist scholars and for advanced students. Alas, its great length, cost, and turgid prose would alienate most beginning students. The volume suffers from several irritating problems. There are numerous typographical errors ranging from consistent misprintings of Spanish words containing the tilde to mangled tables (e.g., Table 5). There is an unusual number of factual errors (e.g., misdating the 1972 Managua earthquake, misnaming Sandino's headquarters, misunderstanding the nature of *zancudismo* in Nicaragua). Such consistent sloppiness distracts the reader and discredits both the publisher and author.

Dunkerley is best when he lucidly describes the Nicaraguan insurrection and revolution and Salvadoran insurrection. Chapter 6 largely overlooks pre-1974 political roots of the Nicaraguan uprising, but is sound thereafter. One strength is the incorporation of recent findings on the class basis of the insurrection, which explain the weakness of the Nicaraguan urban and rural proletariats in contrast to the contribution of the urban informal sector in the revolt. He provides refreshingly clear discussions of the differences among the three Sandinista factions and their reunification in 1978–79 and of the decision of the bourgeois opposition to ally with the Sandinista National Liberation Front (FSLN). Chapter 7 usefully outlines the goals and politics of the FSLN-led revolution through the mid-1980s. This section is frustrating (not Dunkerley's fault) because events change so fast in Nicaragua. Particularly dated already is material on the Contra war, which peaked and waned while *Power in the Isthmus* was in press.

Chapter 8 on El Salvador draws heavily on *The Long War* but updates and revises it. Dunkerley now ascribes more importance to peasant mobilization in the pre-1979 period and somewhat deemphasizes urban mobilization. He analyzes the U.S. strategy of using elections to consolidate the regime and assist the armed force to defeat the Farabundo Martí National Liberation Front. In El Salvador, too, events are rapidly outrunning this analysis.

The book's greatest weakness lies in the critical chapter 5, which attempts to identify the economic forces driving the insurrections in Nicaragua, El Salvador, and Guatemala. Dunkerley correctly argues that the three decades of rapid economic growth after 1950 contributed to the insurrections, but despite delineating key traits of the boom, his data analysis is often unpersuasive. He omits critical available data on real wages and income and wealth distribution in the 1960s and 1970s. With them he could have shown more clearly how changes in income and wealth distribution generated key grievances for major sectors of the working classes of Nicaragua, El Salvador and Guatemala.

Treated only lightly are revolutionary theory, the radical church, and general U.S. economic and political strategy. In attempting to synthesize both solidarity and academic treatments of Central America, Dunkerley engages in much arcane theoretical debate (especially in his notes) likely to interest political theorists, leftist revolutionaries, and academic Marxists, but few others.

In treating his general themes, Dunkerley succeeds better at contrasting "unity with diversity" (explaining the region's similarities and differences) than he does at accounting for "continuity and rupture" in the region's national histories. Despite the virtue of its ambition, this book will probably frustrate specialists on Central America because of its errors and the shortcomings of its analysis of the economic roots of the region's recent unrest. In both analysis and presentation *Power in the Isthmus* falls short of its laudable targets.

JOHN A. BOOTH

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Space Weapons and International Security. Edited by Bhupendra Jasani. Oxford: Oxford University Press, 1987. 366p. $59.00.

This is a collection of 21 articles covering the diverse and complex issues associated with the militarization of space. The volume was organized by the Stockholm International Peace Research Institute in 1985, just as the debate over President Reagan's Strategic Defense Initiative (SDI) was reaching its height and was published two years later. The editor contributes a lengthy opening section that condenses much of the inevitably sprawling and somewhat redundant discussion of the succeeding essays, written by experts from the United States, the Soviet Union, and Western Europe. The bulk of the analysis is devoted to antisatellite (ASAT) and antiballistic missile (ABM) systems, viewed both as defensive and offensive weapons.

There is a considerable measure of agreement among the participants on several points. All agree that foolproof population protection is utopian. The criteria adanced by Ambassador Paul Nitze to justify the massive expenditures needed to develop such a system—survivability and cost effectiveness—cannot be met. The adversary, as physicists Richard Garwin, Kosta Tsipis, and others imaginatively and concisely demonstrate, has too many cheap and readily available countermeasures at its disposal to defeat any ABM system built to protect populations. Even SDI enthusiasts, like Stephen Rockwood of Los Alamos Laboratory and Colin Gray, concede that a resourceful adversary can easily frustrate an ambitious SDI program. Fast-burning missiles effectively nullify X-ray lasers and directed energy weapons. Protective shielding can defeat satellite-positioned lasers; their weight requirements also pose formidable problems for an operational anti-ABM system. The dependency of the system on vulnerable satellites very likely means that they will be destroyed in the first attack wave. The adversary, moreover, need only multiply warheads and launchers faster than anti-ABM systems can be created to identify and destroy them. The task of discriminating between thousands of decoys and warheads during their midcourse journey appears insurmountable. As the British Chevaline program suggests, all of the missiles can be disguised as decoys and too little time remains as they reenter the atmosphere to find and kill all of the incoming decoys and missiles. The ABM system must also work perfectly the first time it is put to a test and within a hostile and unpredictable environment.

The slim prospects of overcoming the obstacles of vulnerability and cost effectiveness narrow the discussion of ASAT and ABM systems to their potentially stabilizing or disruptive impact on deterrence. Only land-based weapons, not populations, can possibly be protected by these systems. While all of the analysts essentially agree on this point, notwithstanding President Reagan's hope for an SDI that would render missile attacks "obsolete and impotent," they disagree on the implications of SDI for deterrence. The essays by Rockwood, Gray, and Frank Gaffney (then U.S. deputy assistant secretary of Defense) present the case for SDI as a deterrence stabilizer or, faute de mieux, as a necessary response to Soviet ASAT and anti-ABM programs. Most contributors are skeptical about these claims for a program that appears costly, risky, and ineffective.

If the volume is excellent in defining the problems associated with space weapons, it is less satisfying in exploring ways that would encourage the superpowers to cooperate rather than accelerate their lethal competition. Some form of superpower mutual protection against third nuclear powers is one area of possible cooperation. Nuclear proliferation threatens the stability of their deterrent regime. The superpowers might well cooperate on a small ABM system, aimed at protecting them against lesser powers, or they might use the threat of building such a system to dissuade other states from going nuclear or from rapidly modernizing their nuclear systems. In this regard, the chapters on British and French nuclear powers and their reactions to superpower cooperation are regrettably weak.

Conversely, more attention might well have been devoted to verification problems to facilitate a superpower accord to limit ABM and ASAT research and development. These are especially nettling, but now urgent, issues; since the climate of 1988 is more favorable than that of 1985 for increased cooperation between the superpowers as a consequence of their decision to dismantle their long-range theater nuclear systems in Europe and Asia.

Despite these reservations, the serious researcher or practitioner concerned with the militarization of space will find this a helpful reference book. Its chapters on the technical aspects of building effective ABM and ASAT architectures are lucidly presented in straightforward prose to assist the nonscientist to gain a firmer technical grasp on the choices facing governments today. The discussion of the political, strategic, and legal dimensions of space weapons is also informed and insightful, written by participants with broad governmental experience and with first-hand knowledge of the intricacies of international arms control negotiations.

EDWARD A. KOLODZIEJ

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The United States and China. By Arnold Xiangze Jiang. Chicago: University of Chicago Press, 1988. 200p. $22.00.

This is a lucidly written interpretative history of Sino-U.S. relations from the early nineteenth century to 1979. Jiang writes within the Chinese historiographic tradition which is concerned primarily with weighing the moral caliber of various actors. He begins with the implicit premise that governments ought to act on the basis of high moral principle and then proceeds to chronicle how the United States' China policy sought instead to further its own economic and political interests. The latter proposition is not difficult to document, and Jiang's work is suffused with indignation at the repeated failure of the United States to act on the basis of moral principle as interpreted by Jiang.

Jiang uses overwhelmingly U.S. sources. He does not delve into the rich Chinese materials on Sino-foreign relations during the pre-1949 period to explain how China's leaders actually saw the world and the choices they faced. In line with this, he dismisses the occasional beliefs of China's pre-1949 rulers that the United States was less aggressive than the other powers and the consequent efforts of those leaders to use the United States to help develop China or to check the advances of more aggressive powers. In fact, according to Jiang, the United States played a leading role in reducing China to "semicolonial" status and in attempting to keep it there. The occasional failures of China's pre-1949 rulers to see things this way Jiang attributes to the general venality, incompetence, and treason of those rulers.

It is virtually impossible for the United States to win by Jiang's standards. The United States is condemned for a willingness to support railway projects in north China at the end of the last century and then for deciding not to support those projects. It is condemned for supplying military assistance to the KMT regime in the early 1930s and then for halting such aid in 1934. Most peculiarly, Jiang condemns the U.S. Open Door policy as a form of imperialist exploitation, yet is full of bitter sarcasm for U.S. unwillingness to risk war with Japan in the 1910s, 1920, and 1930s in order to uphold U.S. rights under that policy.

Jiang assumes that after 1927 China's Nationalist regime was a counterrevolutionary regime of national betrayal. It follows that all U.S. support for that regime was a form of imperialist aggression. He does allow that between 1941 and 1945 U.S. aid to the Nationalists was part of the global antifascist struggle and, thus, in accord with the wishes of the Chinese people. However, the failure of the United States to use this aid to dictate to Chiang Kai-shek a more benign policy toward the Chinese Communist Party (CCP) was, according to Jiang, yet another example of U.S. imperialist policy.

Jiang's treatment of Chiang Kai-shek is overly simple. He depicts Chiang as a craven lackey of imperialism rather than the proud, tradition-bound, anticommunist nationalist that he in fact was. Jiang mentions Chiang's repeated threats to surrender to Japan during the Second World War, for example, but fails to connect those threats with various concessions Chiang won from the United States, like China's inclusion into the Big Four in October 1943 (see John W. Garver's *China and the Soviet Union, 1937–1945* [1988]). To Jiang, Chiang's threats to surrender are merely further examples of Chiang's tendencies towards national betrayal.

Jiang's treatment of the post-1949 period is seriously flawed. During the pre-1949 period, Jiang allows himself to criticize various failures of China's rulers, whereas one looks in vain for some mention of a Chinese miscalculation during the post-1949 period. Misconceptions and distortions abound, however, in Jiang's own treatment of U.S. policy toward China in the 1940s and 1950s. Much of the inflexible hostility which Jiang rightly detects in U.S. policy toward China during the 1950s was not rooted primarily in anticommunist ideology, as Jiang suggests, but in a desire to undermine the Sino-Soviet alliance (see David Mayers's *Cracking the Monolith* [1986]). But perhaps most troubling is Jiang's inability to put himself in the position of U.S. leaders. He reviews various U.S. actions to contain China in the 1950s—diplomatic isolation, economic embargo, military pressure, and so on—but completely ignores various events to which the United States was, in part at least, responding: the CCP arrest of the U.S. consul in Mukden in 1948; the seizure of the U.S. consulate general in Beijing in January 1950; the Chinese-Soviet treaty of 1950; the Soviet–North Korean decision to invade South Korea; China's substantial support for the Viet Minh beginning in late 1949; China's support for communist-led revolutionary movements across Asia; and, perhaps most incredibly of all, the debate between the CCP and the Soviet Communist party during the late 1950s over bloc policy toward the United States. The result is a seriously biased, one-sided account of Sino-U.S. relations.

Yet, ironically, these very shortcomings demonstrate the value of Jiang's work. It *is* extremely difficult for people to view the world from the perspective of people in another culture, and Jiang's book provides a very useful example of how many Chinese view modern Sino-U.S. relations. As such, it should be included in library collections.

JOHN W. GARVER

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Power, Protection, and Free Trade: International Sources of U.S. Commercial Strategy, 1887–1939. By David A. Lake. Ithaca: Cornell University Press, 1988. 241p. $29.95.

Systemic or structural approaches seek to explain both international outcomes, such as cooperation or regimes, and foreign policies in terms of characteristics of the international system as a whole, with minimal reference to the internal characteristics of states. In *Power, Protection, and Free Trade* (as well as a series of important articles), David Lake has built on earlier work by Charles Kindleberger, Stephen Krasner, and Robert Gilpin to advance a more refined and complex structural theory of trade policy. Lake follows previous theorists of hegemonic stability in noting the importance of power—operationalized as share of world trade—in providing the "infrastructure" required to sustain a liberal order, though why *relative* size should be important in this regard remains unclear. According to Lake, relative size also increases the likelihood that countries will be able to alter their terms of trade through an optimal tariff unilaterally. Finally, size is presumed to affect the ability to exploit the open economy multiplier by running current account surpluses, though the most important instruments for achieving this objective are the exchange rate and, ultimately, macroeconomic policy, rather than trade policy.

One of Lake's central contributions is to include a measure of relative productivity in his conception of the international trade structure. The importance of relative labor productivity in deducing trade interests is somewhat ambiguous, however. Differences in unit labor costs across countries are one basis for international specialization and justify free trade policies regardless of relative productivity. Lake argues, however, that medium-sized and small countries with relatively high labor productivity are better positioned to benefit from protection of industries with increasing returns than are other states. Though the empirical evidence on the efficacy of strategic trade policy and trade surpluses remains tenuous, the arguments about increasing returns and the open economy multiplier are crucial to Lake's story. They constitute the systemic source of the mercantilist policy preferences he attributes to the most important nonhegemonic actors in the system, the "opportunist" (high-productivity) and "spoiler" (low-productivity) middle powers.

The next step in the argument is to generate an international structural matrix from power (large, medium, and small powers) and relative productivity (high or low). This yields six basic structural positions, though the lines between categories are necessarily somewhat arbitrary. International structures vary depending on the distribution of countries occupying these positions. Though refinements have been added by Lake's careful specification of structure, the most important distinction empirically is still between hegemonic systems (the United Kingdom until 1912 and the United States from 1945 to 1965) and the systems in which rising or declining hegemons have contended with one, two, or more middle powers.

Lake's dependent variable is trade "strategy," including the general trend of tariff levels but also whether tariffs are negotiated and the degree of discrimination in national trade policies. The final step in the theoretical argument is to show how different structures affect both national choices and bargaining processes. This is done by the application of simple game theory to the interactions of different actors in the structure. The expectations derived from structure alone are indeterminant, however, and can therefore only be treated as broad constraints or as a heuristic. Hegemonic systems may or may not lead to a liberal international trade structure; it depends on other factors, including the resources available and the net cost to the hegemon of providing the international infrastructure, sanctions and side payments required to make such a system work. Conversely, Lake follows Robert Keohane in arguing that the absence of a hegemon need not result in restrictive trade policies if the overall economic environment is stable and institutions and strategies exist to overcome collective action problems.

These qualifications to a structural approach already seem quite significant, but Lake is honest in adding yet another: the influence of domestic structures on policy. It is crucial to the descriptive utility and theoretical integrity of a structural theory that nation-states can reasonably be treated as relatively unified rational actors. Lake adopts a stylized view of the state as consisting of two parts: an executive that responds primarily to external incentives and acts to maximize an aggregate national welfare function dictated by those incentives; and a representative part that is responsive to societal demands.

This conceptualization raises a new theoretical problem, however. Why should we expect that the executive determines policy? As Lake admits, "despite the efforts of the foreign policy executive, the nation-state may choose not to follow its national trade interests" (p. 87). This is tantamount to saying that structure may or may not matter. But even if trade policy outcomes correspond with Lake's structural predictions, they may usually be given an equally plausible and parsimonious domestic explanation: protectionist policies result from emergent or declining industries; strategic policies from those with increasing returns and an interest in foreign markets; and the call for free trade from internationally competitive industries and those with extensive international commercial and investment ties. Moreover, a domestic explanation has the advantage of reminding us that national trade interests are not homogeneous, that contradictory interests coexist, and that these interests are likely to be reflected in the compromises that are struck.

This is particularly plausible since the United States generally has a "weak" state, meaning that policy is likely to be responsive to business pressures. Lake counters that executives have ways of influencing policy outcomes. This is unquestionably true, though executive initiative does not *necessarily* constitute evidence for the influence of international constraints, since executives are also responsive to domestic political constraints. The emphasis on the executive does put us on the terrain of a somewhat different range of domestic political variables than interest group theorists have focused on, however, including tactics, agenda setting, executive coalition building, and the nature of the institutions through which interests are represented. Like interest group pressures, these forces may all offset or reinforce structural constraints.

This is what we find to be the case in the careful reconstruction of U.S. trade policy that Lake provides in the second half of his book. The evidence suggests that presidents are cognizant of international constraints and respond to them, often in advance of the societal pressures filtered through Congress. But we know that firms are cognizant of international constraints and opportunities as well and that trade policy is not simply the result of executive initiative but of a changing domestic economic structure. An alternative formulation might draw the line less sharply between the "international" and "domestic" and focus less on the state and more on how the international structural position of domestic industries and sectors affects trade policy preferences. Such a theory would not necessarily yield sharp, determinate predictions about turning points; but neither does an international structural theory.

There are a few points where Lake's theoretical framework steers him away from contending explanations or leads to odd characterizations. For example, in the chapter on 1887–97, we read virtually nothing about the role of the depression in shaping trade policy. His definition of structure leads him to focus on U.S.-European relations in the post-1975 era, though it is clear that U.S. relations with East Asia have been equally if not more constraining.

Overall, however, Lake has written an excellent book. He is meticulous in showing when and how his predictions are confirmed or disconfirmed by the evidence. His version of structuralism in the trade area is by far the most sophisticated and nuanced we have. By focusing sharply on structure, he alerts us to external constraints and opportunities, and suggests interesting reinterpretations of particular historical events. Lake has probably reached the limits of what such a structural approach can accomplish, however. As Lake argues in closing, the future theoretical task is to build on structural insights, while incorporating more centrally the interaction between the structure given by the interaction of states and the *economic* structure given by firms, markets, and changing comparative advantage.

STEPHAN HAGGARD

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Pollution and the Struggle for the World Product: Multinational Corporations, Environment, and International Comparative Advantage. By H. Jeffrey Leonard. New York: Cambridge University Press, 1988. 254p. $39.50.

Economic theory and journalistic anecdote both suggest that corporate decisions about where to put polluting plants may be influenced by the "natural factor endowment" of different localities. In other words, if corporations anticipate lax pollution controls in Third World or newly industrializing countries, then, other things being equal, that is where they will locate. Leonard attempts a thorough refutation of this contention, and those interested in the topic need look no further for a detailed and competent treatment.

The first two chapters reflect the origins of the book, for they read like the standard literature review section of a dissertation. They constitute a thorough (if indigestible) review of the field of international political economy, with little explicit connection to the book's subject.

This reader's interest was only truly engaged in chapter 3, at which point Leonard splits the initial question into two hypotheses. The first, "industrial flight" is that industries have been leaving the United States and other highly industrialized countries in reaction to increasingly stringent environmental regulations. The second, "pollution haven," is that some countries have used deliberately lax environmental standards to lure new industry. The industrial flight hypothesis is refuted rather decisively by statistics showing that the most pollution-intensive industrial sectors (specifically, chemicals and mineral processing) do not show patterns of domestic disinvestment and foreign investment any different from cleaner industries.

The pollution haven hypothesis, rather murkier to begin with, receives a less-conclusive treatment based on four case studies: Ireland, Mexico, Spain, and Romania. Of these countries, only Ireland ever proclaimed a "dustbin" strategy to attract industry. And even that policy was reversed with Irish economic growth in the mid-1970s, before any major polluters could be attracted. These four cases cannot of course support a decisive refutation of the pollution haven hypothesis, for other havens might exist. Leonard appears to have chosen these four on the grounds that they are the kinds of places United States corporations seem to have been concentrating their overseas investments. But, as he recognizes, the real horror stories of corporate neglect in the Third World and rapidly industrializing countries are to be found elsewhere. Further, Leonard might have extended his case studies beyond the happy year of 1980 (when he did his fieldwork), especially given that the economic convulsions experienced by Mexico, Ireland, and Romania in recent years could conceivably have changed the picture.

The policy recommendations with which the book concludes are consistent with the conservative environmentalism of the Conservation Foundation (whose International Development Program Leonard directs). So the United States should not deregulate, for industry threats to flee are empty. Newly industrializing countries for their part have little to gain by tolerating corporate negligence or by pursuing a dustbin policy. And corporations should err on the side of too much environmental protection, both to please their hosts and to ensure themselves against future political uncertainties (especially popular crackdowns against foreign polluters).

There are, though, some blemishes to mar this portrait of vigorous but environmentally sound economic growth, responsible corporations, and wise governments. These blemishes come in the form of "extreme social blockage by groups pursuing political or selfish goals" (p. 243). In this light, Irish republicans, Basque separatists, and Mexican leftists—not to mention environmental extremists—are guilty of "exaggerating potential impacts and playing upon the fears of uninformed local people" (p. 243). Those less sanguine than the author about the benefits of environmental corporatism might at least lend an ear to these alternative voices.

JOHN S. DRYZEK

University of Oregon

Book Reviews: International Relations

The Fate of Nations: The Search for National Security in the Nineteenth and Twentieth Centuries. By Michael Mandelbaum. Cambridge: Cambridge University Press, 1988. 416p. $39.50 cloth, $13.95 paper.

In his latest book, Michael Mandelbaum explores the impact of the international system on states. He seeks to show how variations in state's security policies are caused by variations in the international system, specifically the organizing principles of the system and the distribution of power within it. He does this by reinterpreting several case studies to show the impact of system on state (what he calls "outside-in" explanations), instead of the more usual "inside-out" explanations, which focus on the influence of internal factors on the security policy of states. The case studies he presents are Great Britain in the nineteenth century, France from 1919 to 1940, the United States from 1945 to 1980, China from 1949 to 1976, Israel from 1948 to 1979, and Japan from 1945 to 1985.

Mandelbaum's book is part of the recent literature on neorealism, which includes Kenneth Waltz's *Theory of International Relations* (1979), Robert Gilpin's *War and Change in World Politics* (1981), and *Neorealism and Its Critics,* edited by Robert Keohane (1986). But there is a fundamental difference between these earlier works and Mandelbaum's. The neorealists are known primarily as theory builders, seeking to construct general explanations of international behavior that will apply to a variety of historical and contemporary systems. While his work is informed by the neorealists, Mandelbaum is interested instead in examining cases of particular states across the past two hundred years. Thus, his book serves as a counterweight to the theoretical efforts of other neorealists, providing empirical referents to evaluate the plausibility and credibility of their arguments.

Mandelbaum balances his cases, selecting states that are strong, weak, and in-between in power. However, it should be noted that all of the states he examines are powerful, ranging from the dominant powers of the nineteenth and twentieth centuries (Great Britain and the United States), to a strong regional power (Israel). Of course, this is the same set of states that is the focus of neorealist theory building, so he has plenty of company in this restriction.

The individual case studies are well done. Each traces the efforts of a state to enhance its national security in the face of a variety of constraints and opportunities. But unfortunately, there is little direct comparison across the cases; there is not even a concluding chapter after the final case study.

Comparisons could have been done in a number of ways. For example, the efforts of France between the world wars to safeguard its borders against Germany could have been compared to the efforts of Israel and China to deal with their security problems. Even more surprising, there is little direct comparison of Great Britain and the United States, two great powers emerging from world wars to become hegemons. Finally, Japan could be compared to the two great powers on economic matters, or to France, Israel, and China on political and military matters.

In sum, this is a well-researched and well-written book and a nice juxtaposition to the abstract and theoretical orientation of most of the neorealist literature. While there is enough information in the book for the reader to make her or his own comparisons across cases, it is unfortunate that the author did not choose to do this himself.

RICHARD J. STOLL

Rice University

Seapower in Global Politics, 1494–1993. By George Modelski and William R. Thompson. Seattle: University of Washington Press, 1988. 394p. $35.00.

George Modelski, in developing his long-cycle theory of global politics, has been one of the leading figures among the international relations scholars who have sought to develop theoretical frameworks capable of explaining historical trends and recurrent patterns in the modern global system. His argument provides an explanation for the long-cycle process whereby a global war gives rise to a dominant world power whose leadership position is based on its use of seapower to control the world's oceans. Over time, as other states increase their naval capabilities, the world power suffers a relative decline which culminates in the next global war. The process takes roughly a hundred years to complete; and over the five hundred years of the modern world system, four states have occupied the leadership role: Portugal, the Netherlands, Britain (through two cycles), and the United States.

Modelski and his colleagues, of whom William Thompson has been the most prominent, have produced a number of studies aimed at developing this theory and at testing empirically some of the propositions derived from the theoretical perspective. *Seapower in Global Politics, 1494–1993* fits into this overall tradition by presenting data regarding the naval capabilities, in terms of the number of major warships, possessed by the four world powers and their major challengers. Readers unfamiliar with the larger project of which the book is a part may see little in the way of a scientific contribution. The book makes virtually no theoretical advances in its own right, nor does it report empirical results by which specific hypotheses can be judged directly. It does serve as a significant contribution to the literature on long-cycle theory, however, in that the picture that emerges tends to support some of the major contentions of long-cycle theorists. The data indicate that the world powers identified by the theory did, in fact, possess an overwhelming proportion of the major warships in the periods following global wars, that the initial advantage declines over time, and that this pattern is repeated in each of the five cycles.

The book contains a wealth of data on the major navies in the modern world system, and it is an important and significant data base. Scholars will find the yearly data on the number of major warships in the service of the global powers as well as the data on naval expenditures extremely useful for testing a number of hypotheses. The data are intended to provide an indicator of global power capabilities, but they could also be used in a number of other contexts including, for example, examinations of naval arms races. The data-gathering procedures and the coding rules are well explained; however, future users should exercise caution when incorporating the data into their own research because of some questionable coding decisions. Two examples illustrate the point. First, a state must possess "5 per cent of the total naval expenditures of the global powers or 10 per cent of the total [major] warships of the global powers" (p. 44) to qualify as a global power. The coding for the early post–World War II era, in which the Soviet Union is considered a global power while Great Britain is not, clearly appears to violate this criterion. According to the data presented, the Soviet Union had no major warships (i.e., aircraft carriers, nuclear attack submarines, and submarine-launched ballistic missile submarines) until 1958 while Britain had a minimum of roughly 15% of the powers' major warships until the same year (pp. 331–34). Second, the coding rule that treats states achieving "global power status in the years between global wars . . . as if they had satisfied the criteria at the conclusion of the preceding global war" provided they exist as independent states and possess some naval capabilities (pp. 44–45) has serious implications. The United States, for example, is considered to have been a global power since 1816, though an observer in 1820 would not have known it. This rule implies that we cannot identify the current global powers until the next global war. If the number of global powers and the precise relationships among them are important theoretically, the inability to identify current global powers hampers our predictive abilities.

These criticisms are intended as a warning to future users and they should not detract from a potentially useful data set. To the authors' credit, the coding rules seem designed to err on the side of including data for too many states in a given year rather than for too few, an intention also reflected in the decision to include data for states identified as great powers in other theoretical frameworks (e.g., Austria). Thus, those approaching the data from a different theoretical orientation should be able to select the data appropriate for their own purposes.

The book also makes a useful contribution to the literature on naval history. The perspective, which stresses the importance of seapower as a determinant of global power and politics, is clearly related to Mahan's; but by placing the arguments in the context of long-cycle theory, some conventional wisdoms regarding the history of seapower are placed within a dynamic model of global politics. The picture that emerges shows not only that a state possessing overwhelming sea power holds a dominant position in world affairs but also that one price of dominance is the eventual erosion of the superiority in sea power on which it is based. In addition, the book fills a void in the naval history literature, which typically focuses on the unfolding of decisive battles or the evolution of naval tactics and technology, by providing some insight into changing force levels. The authors were careful to consider how technological advances affect force levels, and allowed the definition of "major warship" to evolve over time to reflect the changing nature of naval warfare. Since force levels affect strategy and the outcome of battles, naval historians, as well as those interested in long-cycle theory, will find much of interest in this book.

T. CLIFTON MORGAN

Rice University

The Nuclear Dilemma in American Strategic Thought. By Robert E. Osgood. Boulder: Westview, 1988. 138p. $23.85.

Nuclear Strategizing: Deterrence and Reality. By Stephen J. Cimbala. New York: Praeger, 1988. 306p. $39.95.

These two books, despite their dissimilar approaches, significantly contribute to our understanding of the momentous changes and policy tensions wrought by nuclear weaponry. Osgood offers a sweeping overview of U.S. strategic thought, in historical perspective, and is more impressed with the convergence than with the differences among those who have wrestled with the implications of the nuclear revolution. Cimbala presents a more complex strategic debate: he tackles major strategic issues—the problems of arms control, the U.S. commitment to NATO, U.S.-Soviet force and doctrinal asymmetries, U.S. dabbling in limited nuclear options, command and control, weapons technology, ballistic missile defense, targeting, escalation, deescalation, and war termination—crafting a comprehensive critique of the U.S. approach to nuclear weaponry and revealing U.S. strategy to be poorly conceived and grievously divorced from actual policy.

To Osgood, the core of the nuclear dilemma is "the moral (as well as ethical) and strategic predicament of being unable to pursue one course of action without incurring the disadvantages of another"; therefore, "the history of U.S. strategic thought can largely be comprehended as the story of how Americans have tried to cope with this dilemma by rejecting, abolishing, or mitigating it" (p. 1). Osgood reduces the central dilemma to trade-offs between three interrelated strategic dilemmas: making deterrence as effective as possible, minimizing its dependence on nuclear weapons, and limiting and controlling the use of force. Osgood attributes most of U.S. strategy to two rival mitigating approaches to the nuclear dilemma—the minimalist and the maximalist. These two approaches differ in emphasis: maximalists stress the need to achieve a more rational proportionality between U.S. policy ends and means; minimalists, less optimistic about the prospects for limiting nuclear combat and more optimistic about the general changes of avoiding war, opt for a more political, bargaining strategy. Nevertheless, both mitigators seek decreased reliance on nuclear weaponry and limited nuclear options should deterrence fail.

Spurning the direct identification or weighing of approaches, Cimbala opts for an incisive diagnostic of the failings of U.S. strategy. He believes these failings to reside in nuclear strategizing ("what one does when unable to use strategy"), scientism ("a self-contained and self-defeating approximation of science"), and a misconceived "rationality." For Cimbala, U.S. strategy unfortunately records the influence of strategizing in means that are disproportionate to ends and in ends that, in turn, are ill defined; the effects of scientism in an overreliance upon narrow quantitative and technical criteria (i.e., those defining force imbalances) to the neglect of political context and substance; and the effects of a misapplied rationality (and scientism) in the inappropriate borrowing, from economics, of a model of choice that is ethnocentric and places unrealistic information demands on nuclear war participants.

Both of these works are highly readable, insightful, and expertly argued; nonetheless, within their success is also a significant failing, and both books aspire to more than they eventually accomplish. Osgood is at his best in grouping viewpoints and highlighting the similarities and differences among them. Nevertheless, he groups strategists only by obscuring some of their important differences. (I do not believe, for instance, that most minimalists share the enthusiasm for nuclear bargaining and options attributed to them.) Furthermore, by invoking schools of thought that vary largely by degree, he can freely intermix assumptions and policy prescriptions that do not fit comfortably together. Cimbala excels at presenting the richness of the strategic debate, with its wide-ranging implications for weapons development, deployment and employment; but he buys comprehensiveness and impressive detail at the price of coherence, punch, and urgency.

In the end, both authors leave important questions unanswered: To what extent do limited nuclear war strategies actually adopt, rather than challenge, the logic of mutual assured destruction? Can NATO nuclear and conventional forces be simultaneously configured or deployed to deny the Soviets their objectives and trigger a larger strategic nuclear conflagration? (Are not these in fact mutually exclusive objectives?) Does not a denial strategy (underlying proposals for escalation dominance, for instance) rest on a fundamentally different set of considerations and assumptions than limited war bargaining strategies? For what Soviet objectives would a nuclear war be fought? And how might the U.S. force structure and employment policy record these objectives? To what extent is the credibility of U.S. policy ultimately dependent on weapons numbers and uses?

The conclusions of both works suffer accordingly: Osgood offers incompatible solutions that can stand only because they are bereft of underlying assumptions; and Cimbala equivocates on, and eventually sidesteps, important issues. Osgood adopts a combined minimalist-maximalist (though maximalist-heavy) escape from the nuclear dilemma—more options, better weapons. He is not fully sensitive to the nonobvious destabilizing consequences of strategic technology (e.g., for ballistic missile defense) and of combining various strategic precepts (e.g., the prescriptions of a denial and a punishment strategy) and is indifferent to the circumstances that would surround a nuclear engagement or the stakes for which it would be fought. Cimbala offers solutions disconnected from his analysis. He enthusiastically describes hypothetical technology of virtual heroic proportions for nuclear command and control and ballistic missile defense and yet recognizes that its effectiveness is muted by a multitude of system-disabling problems as well as various psychological, political, and sociological forces. While making a compelling case for conventional solutions (in the specific sense) to the nuclear dilemma, he ends up endorsing strategic defense and limited nuclear options, but only tentatively and without clearly explicating the purposes to which weapons systems would be directed or the conditions that would govern their use. Both authors seek the middle ground; however, this ground is too large to straddle, and the "truth" cannot be found between the extremes.

JAMES H. LEBOVIC

George Washington University

Opening Financial Markets: Banking Politics on the Pacific Rim. By Louis W. Pauly. Ithaca: Cornell University Press, 1988. 256p. $29.95.

In the past decade, the structure of domestic financial systems and the behavior of financial actors have been at the center of comparative political economy debates. Louis Pauly's study falls into this tradition. His study analyzes the growing convergence in the regulation of foreign banks in four markets on the Pacific rim—Australia, Canada, Japan, and the United States. Despite distinct financial systems and institutions, governments in the four countries formulated similar standards and policies to regulate market access of foreign financial institutions. According to the author, this convergence in deregulation and liberalization of financial markets should not be attributed to systemic or economic forces but to political will. Economic interdependence does not automatically ensure harmonization of regulatory policies of foreign institutions. It is true, however, that officials in all four countries changed their views on the trade-off between protection and reciprocity with the spread of international norms and goals about new standards for reciprocity and equal national treatment. Nevertheless, technological innovation, economic change, and international norms must first be politically internalized before they influence the reformulation of domestic policies affecting the market access of foreign financial institutions.

Which political catalysts stood behind the change in banking policies? Each case study presents a slightly different configuration of domestic pressures operating in a different regulatory environment. In the United States, the large domestic banks pushed for greater liberalization after foreign banks had penetrated the domestic market by exploiting confusing jurisdictional division of labor within the federal government. The private sector, however, first had to reach internal consensus about the appropriate role of foreign banks and to iron out their own conflicting interests before they succeeded in influencing government officials. Once the banking sector accepted the need for greater federal intervention, the International Banking Act was enacted.

Foreign governments were the primary catalyst in Japan, where banks had expanded abroad aggressively in the 1970s. The success of the Japanese banks prompted non-Japanese banks to lobby their governments to increase pressure on the Japanese government to liberalize their domestic market. The Ministry of Finance was willing to bow to the pressures because it did not want another politicized and complicated international economic conflict.

In both Canada and Australia, government officials and domestic interests have been rather ambivalent about foreign capital because of its critical role in the economic development of both countries.

Despite strong economic nationalism and fear of financial internationalization, the Canadian confederal system left several banking areas unregulated that were subsequently exploited by foreign banks to enter the domestic market. After having created a niche for themselves, foreign banks engaged in relentless lobbying to remove several punishing restrictions on their operations and further expansion. They mobilized support by emphasizing their contribution to the provincial budgets, the labor market, and capital imports. Building alliances with local governments and domestic businesses, while the large national banks were temporarily disorganized, foreign banks managed to convince Canadian government officials to accept multilateral obligations and reciprocity.

Something similar happened in Australia. Traditional hostility and extensive state involvement in economic development by the Australian federal government had kept foreign banks at bay. But the jurisdictional split affecting financial regulation provided foreign institutions the opportunity to monopolize certain specialized banking operations in localized markets. Though foreign banks, as in Canada, were absorbed in the national economy, they did not succeed in building broad alliances with domestic interests. Instead, the governing Labor cabinet in the early 1980s decided that the domestic market would be better served by deregulation and market competition. Within the central government, the emerging consensus was that the regulated and supervised relationship between state and market had become burdensome and costly. A new balance between competition and regulation would increase efficiency, production, and growth. Political leaders, despite domestic opposition, pushed through economic reforms that entailed a more effective regulatory structure with a coherent design for foreign bank access and greater international convergence.

In general, this is a well-organized and eminently readable study. Pauly confesses that the book represents the culmination of a 12-year involvement with the banking industry. This is obvious from reading the case studies and the theoretical framework. In *Opening Financial Markets*, the determinant factors are the attitudes, domestic and foreign alliances, and economic interests of the financial institutions. This book is a valuable contribution for those who want to study comparative financial systems, banking policies, and financial deregulation. Furthermore, the inclusion of Australia and Canada is a welcome addition to the already existing literature.

PAULETTE KURZER

Babson College

Canada's Arctic Waters in International Law. By Donat Pharand. New York: Cambridge University Press, 1988. $49.50.

This volume examines the international legal regime for Canada's Arctic archipelago, with the expressed purpose of clarifying the legal implications for the possibility of increased maritime traffic through the region in the 1990s. The author, who is professor of law at the University of Ottawa, is well known for his comprehensive, comprehensible analyses of Arctic ocean matters over the past two decades. This treatment contributes much to reaffirm that deserved reputation.

The study's central aim is to explore the validity under international law for Canada's assertion in 1985 of claim to straight baselines around its Arctic archipelago. Within this context, Pharand investigates the legal propriety and implications for Canada associated with applying the sector theory, historic waters doctrine, and straight baseline projections to the ocean-bound Arctic. A fourth consideration assesses the legal status of the Northwest Passage as a straitlike international waterway.

In the first part of this study, Pharand thoughtfully evaluates the bases for the sector theory's validity in international law, in particular the support that meridian lines, boundary treaties, contiguity, and customary law have given to the sector theory's application in the Arctic. Relevant court cases are discussed in a fashion easily understandable to the layman and the student. Though not new, Pharand's conclusion remains instructive for international law and Canada's government policy in the Arctic: "*The sector theory has not developed as a principle of customary law,* neither general nor regional, and cannot serve as a root of title for the acquisition of sovereignty, particularly not to areas of the sea" (p. 79, Pharand's emphasis). I substantially concur with that appraisal.

The study's second part addresses the legal implications flowing from the Canadian Arctic archipelago being considered historic waters. Origins of the historic waters concept are traced and its legal status and present application to Canada's northern seas are critically analyzed. Pharand's succinct conclusion regarding the efficacy of the historic waters doctrine to support Canada's claims to Arctic waters is not likely to win comfort in the External Affairs Office in Ottawa, but it is right on point: "Canada would not succeed in establishing that the waters of the Canadian Arctic Archipelago are historic international waters" (p. 125). Neither use of the sector theory nor applicataion of the historic waters concept to Arctic seas furnish much support in international law for Canada's assertion of claim to valid title in the region.

Pharand's treatment in part 3 of straight baselines and the lawfulness of their applicability suggests that greater legal merit resides in their validity for the Canadian Arctic archipelago than the sector theory or historic waters. In January 1986, the establishment of straight baselines around the perimeter of the Canadian Arctic Archipelago entered into force under Canadian legislation. Taking into account state practice and the criteria for properly applying straight baselines set out by the International Court of Justice in the 1951 Anglo-Norwegian Fisheries case—namely, the general direction of the coast, the close link between land and sea, and regional economic interests evidenced by prolonged use—the author submits that Canada's baseline assertion may be considered necessary and legally proper. This will be the case so long as Canada fulfills vital obligations to protect the marine environment, the livelihood of the indigenous Inuits, and the security of its territory. There is little reason to quibble with this conclusion if one accepts the assumption that the island groups north of Canada indeed fall within the sovereign jurisdiction of that state.

The fourth section assesses the legal regime of the Northwest Passage and the likely implications arising from increased maritime transportation of hydrocarbons through the region. Readers familiar with the author's previous works on the Arctic, in particular *The Northwest Passage: Arctic Straits,* will discover that similar ground is covered, albeit the text has been substantially rewritten and, thankfully, updated. Pharand reviews the feasibility of five main routes through the archipelago and then evaluates their potential transit utility. He argues that the legal status of the Northwest Passage turns on whether that waterway qualifies as an international strait as qualified under provisions in the 1982 Law of the Sea Convention. He rejects this possibility for the present, on grounds that Canadian waters fail to meet the functional criterion standard: only 45 complete transits have been made through the passageway during the last century, a record that hardly substantiates the Northwest Passage as a major regional thoroughfare for international maritime commerce. In the end, he submits, the issue of Canada's protecting its waters surfaces as a prominent new legal concern for that state in the Arctic: "The least Canada must do, to protect its newly proclaimed sovereignty, and national security, is to develop an adequate submarine detection capability in the Arctic. Surely it has a right to what goes on in its own waters; indeed, it has a duty to find out" (pp. 242–43).

There is little to disagree with in Pharand's assessment or his conclusions. His analysis is cogent, the main points well argued, and the treatment adequately documented. However, I believe that the study would have profited from an analysis of the status of ice in international law, as well as the difficulties that ice presents for sovereign claims and coastal delimitation. Several relevant and intriguing questions might then have been addressed. For example, what legal and geophysical impediments does ice present for straight baseline demarcation? How does the physical nature of ice affect territorial delimitation in the Arctic? What legal status, if any, do ice islands have in the Arctic, and what opportunities or difficulties do they present for Canada's claims to sovereign jurisdiction in the region? While these issues may lie beyond the present study's intentions, they are relevant and should not be dismissed when considering Canada's Arctic waters in international law.

On balance, this volume stands as a valuable addition to the literature on polar affairs. The inclusion of 22 maps and figures is much appreciated, and these contribute substantially to clarifying sticky legal points affected by the region's complex geography. Pharand has provided a thoughtful and useful treatment that is not overly burdened with convoluted legalistic reasoning. It is a straightforward, direct assessment of the historical circumstances, geographical conditions, and relevant application of international law to Canada's Arctic waters. As such, the volume comes highly recommended for both scholars and students seriously interested in the law and politics of the frozen north.

CHRISTOPHER C. JOYNER

George Washington University

The Path to Vietnam: Origins of the American Commitment to Southeast Asia. By Andrew J. Rotter. Ithaca, NY: Cornell University Press, 1987. 228p. $29.95.

William Fulbright and the Vietnam War: The Dissent of a Political Realist. By William C. Berman. Kent, OH: Kent State University Press, 1988. 235p. $24.00.

Johnson, Nixon, and the Doves. By Melvin Small. New Brunswick: Rutgers University Press, 1988. 322p. $35.00 cloth, $12.00 paper.

A quarter of a century after the Gulf of Tonkin Resolution, the United States remains haunted by the war in Vietnam. The current upwelling of popular interest in Vietnam reflects a widely felt need to come to terms with the memories and legacies of our longest, most divisive, and most bitterly remembered war. The three volumes under review have in common a focus on the domestic politics of the involvement in Indochina. If, as has often been asserted, the war was lost here at home with the collapse of the final domino of U.S. public opinion, these works should enhance our understanding of the political forces that shaped U.S. policy making on the war.

One of the most perplexing questions about the war is how U.S. policy makers came to attach strategic significance to an otherwise remote and peripheral land. Andrew Rotter's excellent book develops original and important insights on this issue. Most students of the subject are familiar with the impact on president-elect Kennedy of Nikita Khrushchev's January 1961 speech spelling out a Soviet strategy to encircle the West through support for "wars of national liberation" in the emerging nations of the Third World. Kennedy increasingly viewed Vietnam as a test case of containing Soviet power by countering the new "peoples' war" strategy. It is Rotter's contribution to take us further back in time and show us the importance of political economy in the thinking of U.S. policy makers. His thesis is that the United States' commitment to Vietnam traces back to the Truman administration's efforts to construct a "liberal capitalist" postwar order (p. 3). Forging such an order would require both the containment of communism and the infusion of U.S. resources to rebuild the war-shattered developed nations and stimulate the underdeveloped nations. In

Rotter's view, "following the summer of 1949, American policymakers found that Southeast Asia was the critical theater in their efforts to contain communism and to effect the recovery of the developed, non-communist nations" (p. 2). That fateful conclusion led U.S. decision makers to a series of commitments that, in the name of halting communism, had the paradoxical effect of "undermin[ing] the ideology of liberal capitalism" (p. 7). Containment and recovery "required American acceptance of colonialism in Southeast Asia. There could be no more dramatic rejection of liberal capitalism than this" (p. 7). Rotter's sophisticated analysis offers much that is new and valuable to anyone searching for the origins of the United States' commitment to Indochina, even those who have read extensively on the subject.

William Berman has written the most thorough account available of Senator J. William Fulbright's complex role in Vietnam policy making. Chairman of the Senate Foreign Relations Committee, sponsor of the Gulf of Tonkin Resolution, and later a leading critic of the war, Fulbright was both celebrated and reviled as a foreign policy dissenter. His underlying policy premises, however, were always well within the parameters of orthodox U.S. thought. Berman's well-researched study is full of new insights on the details of Fulbright's evolving role. Berman conducted 30 interviews with Fulbright and his contemporaries, consulted published and documentary sources, and made good use of the voluminous Fulbright papers at the University of Arkansas. The result is a rich, nuanced portrait of themost famous postwar legislator at some of the pivotal episodes of his career.

We see more fully, for example, the considerable effect of personal loyalties and animosities in the policy process. Berman provides the best account yet of how skillfully Lyndon Johnson manipulated his old friend into sponsoring the Gulf of Tonkin Resolution in August 1964. As he puts it, "Fulbright was eager to remain on the inside, and Johnson, no doubt sensing Fulbright's vulnerability, flattered him into believing that he was a wise and trusted counselor" (p. 25). By early 1966 Fulbright had become one of the leading congressional critics of Johnson's war policies. While there is no reason to doubt the sincerity of Fulbright's dissent, Berman demonstrates that it was driven in part by "Johnson's harsh and hostile treatment in the fall of 1965," occasioned by Fulbright's criticism of the intervention in the Dominican Republic (p. 47).

Berman clearly endorses Fulbright's critical stance, and it is this close identification with his subject that creates the book's principal problems. In celebrating Fulbright's evolution as a dissenter, Berman uncritically accepts Fulbright's sometimes simplistic analysis of the realities of Southeast Asia. A number of old myths are simply reproduced, including the canard that "the war was civil in character as most of the Viet Cong fighters in the South were from that region" (p. 52). As Stanley Karnow has shown, it was a "myth, in which many Westerners then believed, that the Vietcong was essentially an indigenous and autonomous insurgent movement" (*Vietnam: A History* [1983], 401). On balance, though, Berman's volume is a valuable addition to the literature on the domestic politics of the United States' role in Vietnam.

Melvin Small's study arose, he tells us, from his own uncertainty of the practical efficacy of the antiwar movement, in which he had been "a foot soldier" (p. xi). He thus set out "to evaluate the impact of the demonstrators, petition signers, and editorial and letter writers on the policymaking process during the Vietnam War" (p. xii). The resulting work is, on its own limited terms, a meticulous piece of scholarship. In addition to standard published accounts, Small uses archival materials and interviews with 40 key figures from the Johnson and Nixon inner circles.

Some may think it curious that so much effort was invested in demonstrating what few would question. To argue that the relentless campaign of disaffected academic and media elites "eroded support for Johnson and Nixon, especially among college students at the best universities, their parents, and members of the attentive and informed public" is to push against an open door (p. 227). Small clearly equates peace with the retreat of U.S. power. This explains the book's exceptionally narrow focus. To Small, it is sufficient to establish that the antiwar movement was instrumental in forcing the United States to withdraw its troops in 1973 and cut off aid to Cambodia and South Vietnam in 1975. It will fall to other scholars with broader agendas to deal in a more balanced way with the complex legacy of the peace movement. The United States' withdrawal did not, of course, bring peace to the people of Indochina, but rather more fighting, the imposition of communist repression throughout all of Vietnam, the genocidal slaughter of a million Cambodians by the triumphant communists there, and the desperate flight of more than one million boat people. This cannot be the kind of peace that the antiwar movement had in mind, but Small's cheerful tale of youthful rebellion does little to help us understand the fate that befell the people of Indochina when the United States withdrew from a war it grew tired of.

Perhaps the generation of scholars that came of age during the Vietnam era will never be able to look at the war with dispassionate objectivity. In addition to the lingering emotions occasioned by the war, there is the seldom-acknowledged fact that only a handful of U.S. academicians actually served in the war. Most academics who were then of draft age avoided the war through student deferments and, later, through occupational deferments granted to educators. This means that there exists a substantial cohort of professors, now at midcareer, who might reasonably be thought to have a vested interest in perpetuating the old and negative stereotypes about the United States' purpose in Vietnam and the men who served that purpose. Even to raise the issue is to invite the wrath of one's colleagues. In the end, however, it behooves us as members of a scholarly discipline to begin to address our institutionalized difficulties and biases in dealing fully and honestly with a war that will not go away.

EUGENE BROWN

Lebanon Valley College

The IMF and Third-World Political Instability: Is There a Connection? By Scott R. Sidell. New York: St. Martin's Press, 1978. 82p. $39.95.

Sidell undertakes a quantitative study of a relatively neglected topic: the systematic relationship between economic austerity programs supported by the International Monetary Fund (i.e., conditionality) and political instability in Third World nations that implement these programs. The principal question around which his analysis converges is, Do "Fund-supported [conditionality] policies actually promote [or exacerbate] political instability" in the Third World? (p. 1). He concludes from his empirical findings that "a Fund-supported stabilization program does not significantly influence the political environment of recipient nations" (p. 7).

The author proceeds carefully and rigorously in configuring his variables and models so as to arrive at the best-unbiased-parameter estimates of the effects of conditionality on political environments. Political instability is operationalized using three dependent variables: collective protests, internal war, and attempted coups d'etat. Each is regressed on various explanatory variables, which include the two independent variables of primary interest (a measure of Fund-supported economic intervention in any given year, and a measure of the historic degree of Fund intervention a nation has experienced); as well as a control group of sociopolitical variables that have all been linked to political instability by previous empirical studies.

Two sets of estimation techniques are used to explore the various relationships that link this constellation of variables: bivariate and multivariate regression using ordinary least squares (OLS) estimates, and bivariate and multivariate regression using generalized least squares (GLS) estimates. Both techniques employ impressively large sets of data: the former makes use of data from 99 nations (891 cases), while the latter considers data from 60 nations (540 cases). In the first set of tests, parameter estimates linking Fund conditionality to political instability tend to be statistically insignificant at the .05 level both in the bivariate (with one exception) and multivariate models. However, two parameter estimates in the multivariate models linking conditionality to internal war and collective protests are significant at the .07 level. The author contends that a spurious causal relation underlies this covariation. The Fund, he argues, is most likely to lend to nations that are already experiencing politically destabilizing economic conditions. The findings with respect to the sociopolitical control variables generally confirm the findings of previous empirical tests on the causes of political instability. Especially compelling is the one disconfirming finding—that political instability is negatively rather than positively related to population growth. This would appear to support public choice–collective action theories of political protest and revolution.

The second set of tests show even less support for the hypothesis that Fund intervention leads to political instability. Interestingly, the only statistically significant parameter estimate linking political instability to Fund intervention (in this specific case, a two-year lag variable) has a negative sign, suggesting that Fund intervention in period *t* actually has politically stabilizing effects in period *t* + 2. Estimates for the sociopolitical control variables tend to show the same signs under GLS estimation as they do under OLS estimation, but they also tend to be smaller and statistically insignificant. The author attributes these latter outcomes to the differing estimation techniques while averting the validity of the causal link between these variables and political instability.

The author concludes by acknowledging various important empirical limitations of the study (such as changing sample sizes from one set of tests to the next, and not estimating the models with the inclusion of lagged dependent variables as explanatory variables) and goes on to suggest paths for further research. One such path would lead to rigorous study of the relation between the Fund's extended facilities (which are more austere and structural than stand-by arrangements and political instability.

On a whole, the book offers extensive quantitative analysis but little in terms of interpretation. Greater emphasis is placed on reporting the findings than on evaluating their theoretical significance in any kind of complex fashion. Furthermore, the theoretical context within which the author works is fairly restricted. Broader theoretical issues regarding financial interpenetration and interdependence in a North-South context tend to be underplayed or ignored.

The normative (i.e., policy) value of the book is limited by the exclusion of economic variables (e.g., growth, inflation, unemployment) from the models. Understanding the direct and indirect effects of economic processes on political environments would be invaluable in indicating how austerity programs can be optimally configured by the Fund (i.e., programs that maximize adjustment while minimizing economic and political risk). Citing the paucity of economic data as justification for this exclusion, the author does acknowledge this limitation and calls for more comprehensively specified models in future research projects.

The book presents some interesting and important empirical findings on a controversial subject. In doing so, it provides some hard knocks for erstwhile critics of Fund-supported adjustment schemes. By the author's own admission, however, these findings are suggestive rather than definitive, and the approach an initial step toward a more complete understanding of the effects of Fund austerity policies on underdeveloped nations. With this caveat in mind, Sidell's book appears to be a valuable contribution to a promising area of research.

GIULIO M. GALLAROTTI

Wesleyan University

Geopolitics in United States Strategic Policy 1890–1987. By Geoffrey R. Sloan. New York: St. Martin's Press, 1988. 255p. $35.00.

When Richard Ashley writes of the "poverty of neorealism" in his *International Organization* (1984), he focuses in part on how political science has isolated itself from the other social sciences. An infatuation with power at the expense of discourse has resulted in research that is very successful on its own terms but has less and less to say to others within the academy in general and the National Academy of Science in particular. Geoffrey Sloan's book on the evolution and relevance of geopolitical thought is an exemplar of this tendency. His thesis is directed single-mindedly to the impacts of geopolitical ideas on U.S. strategic policy, impacts that, he has assured us from the outset, must logically exist, albeit in an indirect way. In the process of telling his story, a number of things are jettisoned, many to do with the historical unfolding of ideas. We learn, for instance, that geopolitics is "one of the 'grand theories' of International Relations" (p. viii), which must come as a surprise to many readers of this journal and to most political geographers, who thought that they had been the ones to popularize the term, and the ones to suffer the opprobrium when German *Geopolitik* was in disgrace.

The book is divided into two parts and consists of eight chapters. After a provocative introduction, the first chapter, "Political Geography and Geopolitics: The Words and History of Ideas," lays out the early terrain of the subject, notably Mackinder's writings. Chapter 2, "Theory in Action: German Geopolitics and the Third Reich," examines the controversial theories of Haushofer and their use by the Nazi party. The third chapter, "Geopolitical Thought in the Postwar Period," provides a brief assessment of the subject's eclipse after 1945 and concludes the first part of the book. Chapter 4 shifts explicitly to the United States, and "Mahan to the Second World War" examines the evolution of U.S. foreign policy. Chapter 5, "Geopolitics and the Containment Era," continues the chronological development, which is brought to the end of the Carter years in "Geopolitics and Contemporary Foreign Policy." Chapter 7, entitled "US Strategic Policy, Geopolitics and World Order," provides a positive assessment of the links between all three phenomena.

Sloan's message is that geopolitics constitutes an important dimension to the U.S. conception of world politics. However, his argument is undermined in a number of ways. The most crucial of these is his definition of geopolitics as a theory of "spatial relations and historical causation" (p. 15), which was borrowed by Sloan from an obscure inaugural lecture a quarter of a century ago. Much better definitions are overlooked, and this is unsurprising, given that there are no citations in the book that postdate 1981 (which means too that the book's title is unfair advertising). A veritable flood of scholarship within political geography has been ignored, including the establishment of the journal *Political Geography Quarterly*, two excellent biographies of Halford Mackinder and one of Bowman, and the beginnings within Germany of a deconstructive period of critical analysis of *Geopolitik*. Nor should we be allowed to ignore the popularization of geopolitical thinking by writers as diverse as Theda Skocpol, Anthony Giddens, and Henry Kissinger (of whom only the last is cited). And, as if this were not enough, there is also Gaddis's 1982 book on geopolitical codes, which covers much of the same ground as Sloan's own work but remains unacknowledged. (For citations to all these writings, see Leslie Hepple's excellent summary, "The Revival of Geopolitics," *Political Geography Quarterly* supplement [1986], pp. 21–36.)

Sloan provides a reasonable historical overview of U.S. interpretations of, and involvement in, global affairs, and he indicates the moments at which strategic thinking and geopolitical considerations could be said to have been united. Yet his cumbersome definition of geopolitics leaves him to question repeatedly whether Kissinger's use of the term was congruent with Mackinder's. In consequence, he misses the more basic point, which is the way in which language imprisons policy discourse. Although some have unkindly suggested that Kissinger meant ego-politics when he spoke of geopolitics, he was emphasizing the importance of thinking—and acting—with a geographical imagination, that is to say, paying attention not just to the global scale but to its component parts and their geostrategic importance (to introduce a term from Cohen's *Geography and Politics in a World Divided* (1963) that Sloan also overlooks. Of course, he did not claim to be using Mackinder's or Spykman's or Mahan's blueprints: he was groping for a new term to describe a significant shift in U.S. capabilities and of necessity retreated to an old one. This is hardly the first time that intention and reality have become confused: certainly, for the Soviets, *geopolitics* will long be a code word for *Geopolitik*.

Sloan presents a reasonable summary of the Anglo-American literature on international strategy and political geography prior to the Reagan years; however, much of the interesting work on geopolitics has been written more recently. With a large number of quotations reproduced within the text, the book has inevitably a disjointed air about it, a feeling that is underlined by erratic copy editing. The reader may also make the choice of spending $35.00 for this edition or $54.15 for the identical Simon and Schuster product. Caveat emptor.

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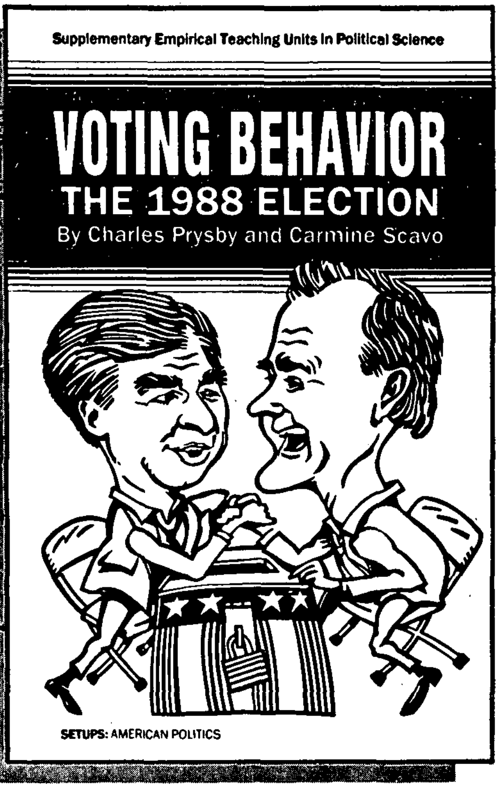

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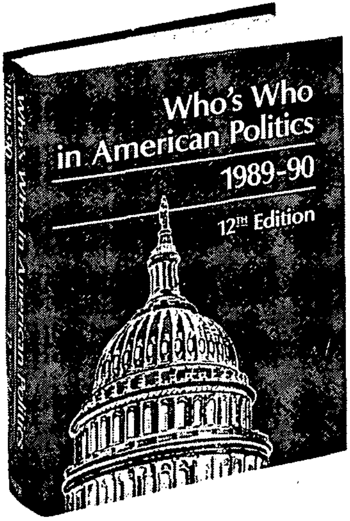

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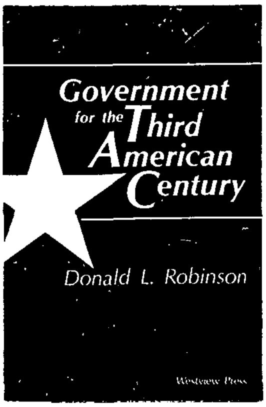

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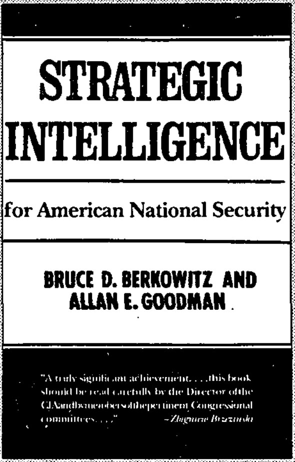

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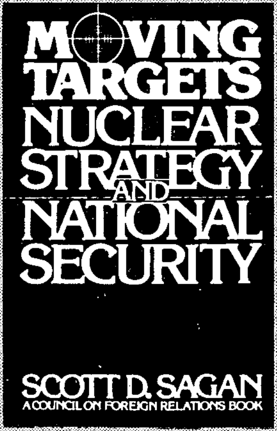

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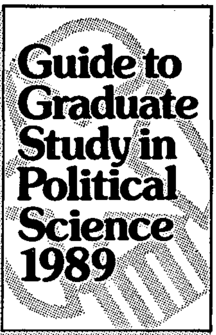

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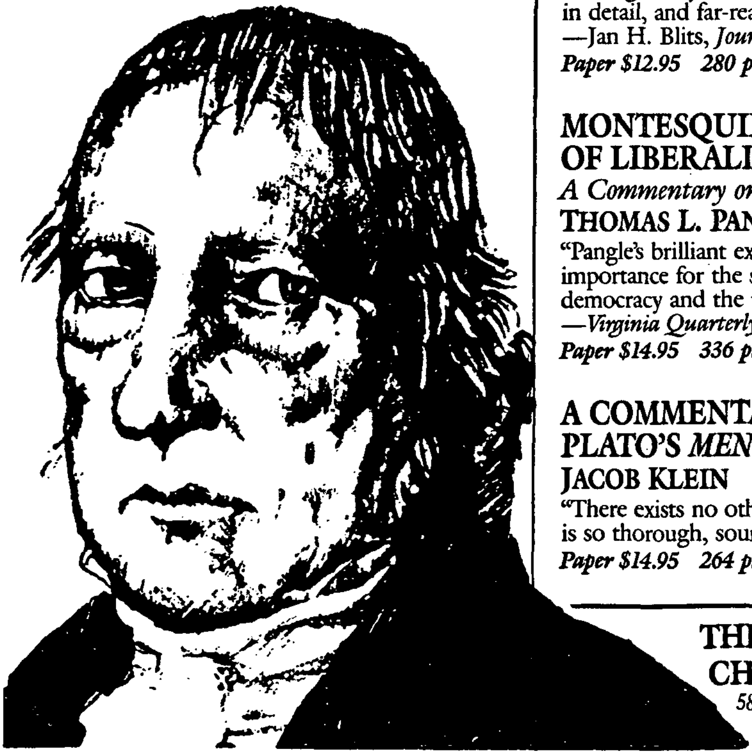

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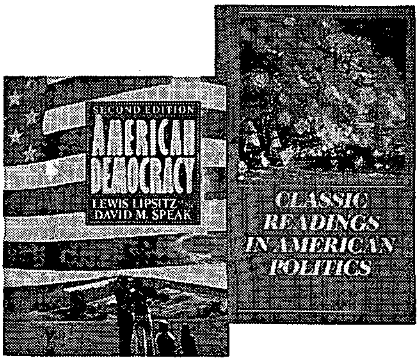

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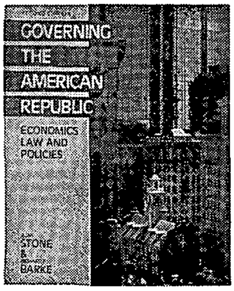

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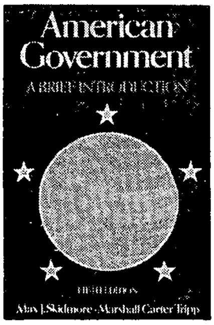

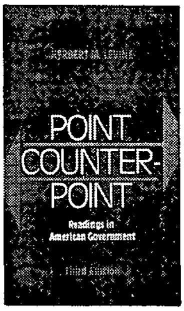

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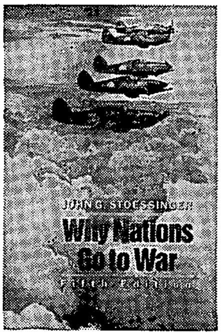

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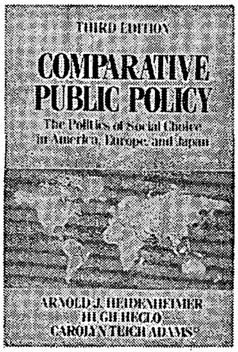
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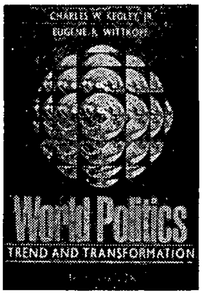

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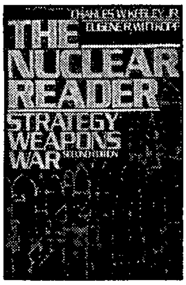

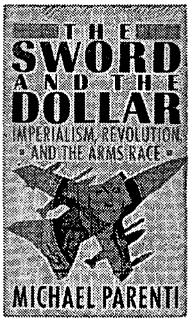

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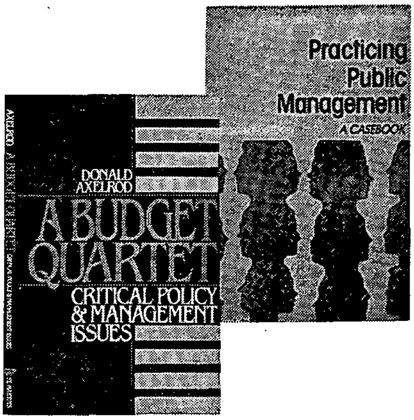
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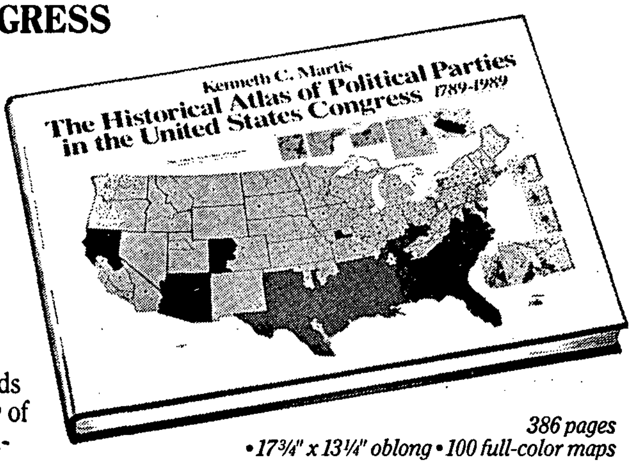
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